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VOL. II

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ABSTRACTS

OF

THE PAPERS

PRINTED IN THE

PHILOSOPHICAL TRANSACTIONS

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ABSTRACTS

OF

THE PAPERS PRINTED IN

THE PHILOSOPHICAL TRANSACTIONS.

Additional Observations on the optical Properties and Structure of heated Glass and unannealed glass Drops. By David Brewster, *LL.D. F.R.S. Edin. and F.S.A. Edin. In a Letter addressed to the Right Hon. Sir* Joseph Banks, *Bart. K.B. P.R.S.* Read November 10, 1814. [*Phil. Trans.* 1815, *p.* 1.]

IN this continuation of the author's remarks on Rupert's drops, he observes, that when they are made of colourless glass, and are held before a sheet of white paper, even the unbroken drops exhibit some appearances which he considers as marks of a crystalline texture; and when they are broken in such a manner that the parts are retained in their relative situation, the structure shows a cleavage in the direction of lines diverging from the apex, or rather in the form of conical layers, having a common axis, with their apex towards the blunt end of the drop.

Since the surface of these drops is cooled suddenly by plunging into water, and fixed before the central parts are contracted, the author conceived that their specific gravity might be less than that of annealed drops; and indeed a difference was found upon trial, amounting to $\frac{1}{55}$th part in the aggregate bulk of its external dimensions; but it appeared, on examination, that the contraction of the glass had taken place from within outwards, so as to leave cavities in the interior sufficient to account for the difference of weight, without supposing any intrinsic difference of density in the glass itself.

Dr. Brewster further observed, that by heating a drop to redness, and allowing it to cool gradually, these cavities disappeared entirely, so as to prove that they did not arise from included air, but merely from the internal contraction of the glass.

Since considerable difficulty occurs in preserving unannealed drops of flint glass, the author has paid attention to the circumstances most favourable to their preservation, and recommends removing them

from the water in which they have been plunged, as soon as the redness in the centre of the drop ceases to be visible.

Since the smallest portion of any polarizing crystal polarizes or depolarizes light according to its position, the author expected to find the same property in the fragments of a broken drop, but upon trial they did not appear to possess this property.

Of the many important conclusions to which the author thinks that these experiments are calculated to conduct us, there is one which he considers too palpable to be passed over, namely, that when the particles of glass are separated to a certain distance by the expansive agency of heat, they assume a crystalline arrangement, which would not be discovered but by fixing them in this state by sudden cooling; since the gradual approximation of the particles, by slow cooling, entirely destroys the crystalline structure thus produced.

In a note the author remarks, that on more than one authority steel is said to be less dense after being hardened by quenching than before, which he ascribes, as in glass, to the sudden induration having commenced at the surface. And he takes occasion to suggest the possibility, that under these circumstances moderate changes of temperature may not occasion any degree of expansion, and that we may obtain, within certain limits, a substance of invariable length that may be useful for pendulums.

Description of a new Instrument for performing mechanically the Involution and Evolution of Numbers. By Peter M. Roget, *M.D. Communicated by* William Hyde Wollaston, *M.D. Sec. R.S.* Read November 17, 1814. [*Phil. Trans.* 1815, *p.* 9.]

The present instrument depends upon a new extension of the principle of the common sliding-rule; for as in that numbers themselves are multiplied or divided by the mechanical addition of their logarithms, so in this their logarithms are multiplied or divided by mechanical application of corresponding logometric spaces.

In the common tables of logarithms, that of 10 is 1, and those of its simple powers are 2, 3, 4, &c.; so also the logarithm of the square root of 10 is $\frac{1}{2}$, or ·5; the fourth root is $\frac{1}{4}$, or ·25, being a decimal index expressing a power of 10 less than unity. In the same manner all other numbers are considered as powers of 10, and their logarithms are integral or decimal indices of those powers.

In the common sliding-rules the divisions are so placed as to mark intervals that are proportional to these indices; so that by simple juxtaposition the sum or difference of any two indices, and consequently the product or quotient of any two numbers, appears by inspection.

In this manner, by addition of two equal logometric intervals, the square of any number may be found; but the instrument so constructed is not prepared to give the higher powers, without proportionally frequent repetitions of the same process, which gives at length a multiple of the index by the tedious operations of repeated addition.

The instrument contrived by Dr. Roget, is constructed to answer this last purpose, with the same facility as common multiplication and division are performed by the common sliding-rule.

For in the same manner as numbers are considered as powers of 10, so their indices, whether integral or decimal, being, in fact, numbers, may again be regarded as powers of 10; and their secondary indices, or logarithmic logarithms, may be laid down as logometric intervals, to which other logometric intervals may be added or subtracted mechanically, so as to present to view any multiples or aliquot part of a logarithm, and consequently any powers whatever of the number to which that logarithm is index; for when the unit of Gunter's line, on the slider, is applied to any number on the scale, divided into logometric logarithms, then 2 on the slider corresponds to the square, 3 to the cube, &c. of the same number.

The author enumerates various uses to which such an instrument is applicable. To all cases of geometrical progression the application is obvious for finding the common ratio, the number of terms, or any particular term in the series. An approximate solution is thus given to all questions of compound interest, to regularly progressive increase of population, and to many calculations of chances. To cases also of the reduction of temperature which a body undergoes by communication to a surrounding medium; and to successive stages of exhaustion, by an air-pump, it may be applied with equal advantage.

Since the scale of the instrument presents to view the proportion of logarithms to each other, while the slider represents the ratios of their respective numbers, it becomes a means of illustrating many points relative to the general theory of logarithms, whether to exhibit a series of logarithms formed according to any particular modulus, or by an inverted position of the slider to find the moduli of all different systems, and accordingly in that, for instance, in which the modulus is equal to the basis.

In this inverted position of the slider it affords a ready answer to various exponential equations, which do not admit of any direct solution, as, for instance, if $x^x = 100$. Let the unit on the slider be placed opposite to 100 on the rule, then 2 will be found opposite to 10, which is the square root of 100; 3 will be opposite to 4·641, which is the third root of 100; and by attending to the decreasing numbers which correspond to increasing numbers on the slider, it will be seen that 3·6 is the point at which they appear equal, showing that $3{\cdot}6^{3\cdot6}$ is nearly equal to 100, and is therefore approximately the root required.

The author concludes by pointing out various forms in which such an instrument may be constructed; since the line divided into logometric logarithms may be turned into a spiral, or arranged according to any other of the various modifications that have been given to the common logometric line of Gunter.

Experiments on the Depolarization of Light as exhibited by various mineral, animal, and vegetable Bodies, with a Reference of the Phenomena to the general Principles of Polarization. By David Brewster, *LL.D. F.R.S. Edin. and F.S.A. Edin. In a Letter addressed to the Right Hon. Sir* Joseph Banks, *Bart. K.B. P.R.S.* Read December 15, 1814. [*Phil. Trans.* 1815, *p.* 29.]

When a ray of light has been so modified by reflection or refraction that in certain planes it is not divided into two parts by a prism of Iceland spar, that ray is said to be polarized; but it may again, by several means, be rendered divisible, and is then said to be depolarized. The object of the author, in this letter, is to comprise experiments on the depolarizing properties of a great variety of substances at the same time, and thence to deduce the general principles on which the various degrees or modes in which they exhibit this property depend.

Dr. Brewster has already, in a former communication, described the general phenomena of depolarization by mica, calcareous spar, topaz, and other regularly crystallized bodies, which have two neutral axes at right angles to each other, and two depolarizing axes also at right angles to each other, but making angles of 45° with each of the neutral axes.

Of the bodies now enumerated by the author, some have no polarizing or depolarizing properties, as rock salt, fluor spar, and spinelle ruby. Out of seven specimens of diamond, four did not depolarize; one depolarized about one fifth of the light, one about one half, and one nearly all the light in every position.

The next class of bodies subjected to experiment by the author, are vegetable and animal substances, from which he expects to deduce important conclusions.

Gum arabic depolarizes light in every position, unless in extremely thin chips. Cherry gum also depolarizes light, with some appearance of a neutral axis. Caoutchouc has the same property; and though it loses it when fused by heat, it recovers the same power gradually in the course of a few weeks. White wax, melted between two plates of glass, depolarizes in every position. Bees' wax has the same property, as may be exhibited by inclosing a portion of a cell of a honey-comb between layers of Canada balsam.

Manna, camphor, and balsam of Tolu were tried with the same results. Various fibrous vegetable substances have also the property of depolarizing, but have neutral axes in the direction of their fibres.

Adipocire from various sources, spermaceti, and soups of all sorts, depolarize in every position. Various kinds of hair, wool, feather, and silk depolarize, but have neutral axes in the direction of their length, and at right angles to it.

Human cuticle, human nail, and various kinds of horn; the cornea of the eye of a man, cow, or fish; a piece of bladder, isinglass, or glue, depolarize in all positions. Certain other animal substances, which are distinctly fibrous in one direction, exhibit neutral axes in

that direction, and at right angles to it, though they depolarize in other directions.

Acetate of lead, confusedly crystallized between two plates of glass, depolarizes in all positions. Plates of ice have in general the same effect, though some exhibit neutral axes.

Oil of mace being a soft solid, opake from confused crystallization. depolarizes also in all positions; but it also exhibits, through a very thin margin, a peculiarity not observed in any of the preceding experiments. When the flame of a candle is viewed through it, the flame appears surrounded by a halo; but if the light be polarized before it is transmitted through the oil of mace, then the flame has four wings or luminous radiations, at right angles to each other; and accordingly if two pencils of light be received at the same time by transmission through Iceland spar, then there are two such images, with their four wings transversely situated, so that the rays of one image correspond in position with the blank spaces of the other.

A slice of tortoiseshell, which also depolarizes in every position, exhibits also, by the polarized light of a candle, faint luminous rays, similar to those seen by oil of mace.

The author next classes these bodies according to the various degrees in which they more or less perfectly depolarize, and more or less perfectly possess neutral axes, in which depolarization does not take place.

The simplest case of depolarization is that effected by a thin plate of Iceland spar, or other regularly crystallized body, the principal section of which is not in the plane of polarization, or at right angles to it, and consequently occasions the polarized ray to be subdivided into two others transversely polarized, according to the original observation of Huygens. Hence if other bodies, as hair, wool, silk, &c. have neutral axes or planes, in which a transmitted ray retains its polarization, while it is depolarized in other positions, this affords optical evidence of the regularity of their internal texture; and though they cannot be called doubly refracting crystals, yet the author conceives that they form two images, which are coincident, but differently polarized, and accordingly that these bodies should be called doubly polarizing crystals.

Other bodies, on the contrary, like the confusedly crystallized acetate of lead, having axes in all directions, present no neutral axes, but depolarize in every direction; while others, according to the degree of their crystalline texture, have the property of depolarizing a greater or less proportion of the incident light, or, according to the degree of regularity of that texture, may exhibit some appearance of neutral axes.

With regard to oil of mace, Dr. Brewster observes, that since the continuous halo which surrounds the flame of a candle seen through it, is divided, by refraction through a prism of Iceland spar, into two sets of luminous radiations surrounding the two flames seen through it, having the luminous rays of the one corresponding in position to the vacant spaces of the other, he infers that the halo itself, in fact,

consists of eight luminous octants alternately polarized in a different manner; and observes, that if we knew in what way the halo is formed, there would probably be no difficulty in explaining these remarkable phenomena.

On an ebbing and flowing Stream discovered by boring in the Harbour of Bridlington. By John Storer, *M.D. Communicated by the Right Hon. Sir* Joseph Banks, *Bart. K.B. P.R.S.* Read January 19, 1815. [*Phil. Trans.* 1815, *p.* 54.]

In the year 1811, with a view to making certain improvements in the port of Bridlington, an examination was made of the depth of a stratum of clay, and another of gravel, at some distance below high-water mark. At low water the harbour is dry, and at high water it has from fifteen to seventeen feet of water in the deepest part. The spot fixed upon for boring, has about six feet of water in ordinary tides.

After boring through 28 feet of clay, and subsequently through a mixed stratum of chalk and gravel, the augur was found to strike against a solid rock, on which that instrument could make no impression, and the work was discontinued, without any appearance of water at that time rising in the bore. But in the course of an hour or two it was observed to be filled to the top with very limpid fresh water, which, after a short time, was projected some inches above the summit, in a stream equal to its calibre. As the water was found to be fit for washing, and all culinary purposes, the bore was first secured by an elm stock ten feet long, and perforated with a three-inch augur, through which was passed a copper tube of the same diameter, well tinned to the depth of 32 feet, and which thus reached from the bottom of the elm stock to the solid rock, in order to preserve a supply of water for the use of the town.

The stream, however, is found not to be constant, but to cease regularly when the tide has fallen to a certain distance, and not to flow again till the tide returns to the same level, its force increasing regularly as the tide advances; and it may be observed to be propelled with much force, even after the bore is overflowed by the tide. By attention to the height of the tide at the time that the water begins to flow, it is found to be very regularly 49 or 50 inches below the level of the top of the bore, excepting after any very unusual fall of rain, when the water has been known to flow even when the tide has fallen as much as eight feet below the top of the bore.

These appearances, says the author, seem not to admit of any satisfactory explanation, without supposing some subterraneous communication between the water of the sea and that of the spring; and Mr. Milne, under whose direction the work has been conducted, conceives the stratum of clay, through which the water issues, to extend over the whole bay in front of the harbour, as far as to Smithwick Sand, which is known to be supported by a ledge of rock that has an almost perpendicular face, and very deep water beyond it. It is

through the fissures of [illegible], till overpowered by the increasing [illegible], which, by its greater specific gravity, will overcome [illegible] will be [illegible] the point at which [illegible] in proportion to the difference of their [illegible].

In confirmation of this hypothesis [illegible] very stormy weather, when there is an unusual swell upon the coast, the water is discharged with an evident [illegible].

Dr. Stoner, however, observes, that the relative altitude to which the spring is elevated after much rain, rather militates against its correctness; and he would expect the additional pressure of the column of spring water at such times to produce an opposite effect, by enabling it to overcome the same column of sea-water during a longer period in each tide.

As it seemed probable that the subject may be elucidated by an acquaintance with the peculiarities of the springs in the neighbourhood, the author remarks, that upon the Wolds around Bridlington there is very little water during summer and autumn, but in the course of two or three weeks after the commencement of frost, the springs begin to run copiously, and in some instances even with considerable impetuosity.

On the Effects of simple Pressure in producing that Species of Crystallization which forms two oppositely polarized Images, and exhibits the complementary Colours by polarized Light. By David Brewster, LL.D. F.R.S. Edin. and F.S.A. Edin. In a Letter addressed to the Right Hon. Sir Joseph Banks, Bart. K.B. P.R.S. Read January 19, 1815. [*Phil. Trans.* 1815, *p.* 60.]

The author having, in former experiments on the depolarization of light by a mixture of resin and bees' wax, had reason to suppose that the effects were modified by pressure, now examines the effects of pressure on a class of substances which, from their elasticity, will restore themselves after removal of the force applied, and thence admit of greater variety in the repetition of the experiments. By employing animal jellies, he had an opportunity of giving them any degree of tenacity that might be wished.

A small cylinder of jelly being placed upright between two plates of glass, had at first no power of depolarization. By gradual drying at its circumference, it soon began to depolarize at that part; and as it became thereby more dense than at its centre, it had the power of a concave lens. At the end of three weeks it seemed dried to the centre, and had then lost both these properties; but by forcible pressure, which it could now bear without injury, it depolarized completely during the continuance of the pressure; but upon its removal, says Dr. Brewster, it resumed its uncrystallized state.

The author next employed isinglass jelly, brought nearly to the consistence of caoutchouc, which, after standing one day, had acquired the depolarizing power even when cut into thin slices; and

when forcibly compressed between two plates of glass, it also exhibited beautiful colours, that were complementary to each other in the two images of a candle seen through it, by means of a prism of Iceland spar; and when the pressure was removed, these complementary tints disappeared.

Inasmuch as these colours might be supposed owing to the thinness to which the plate of jelly was reduced by pressure, Dr. Brewster cut the cake to the same thinness which it had possessed while under compression, but without any production of colours till pressure was again applied.

In the author's concluding experiment, he formed one twentieth of an inch thick of the same jelly, by melting it between two plates of glass. When merely consolidated by cooling, this had no power of depolarization; but by pressure it instantly restored the evanescent image, and exhibited, as in the former cases, the complementary colours, showing, says the author, that pressure communicates a modification of structure correspondent to that of crystallized minerals.

Experiments made with a View to ascertain the Principle on which the Action of the Heart depends, and the Relation which subsists between that Organ and the nervous System. By A. P. Wilson Philip, *Physician in Worcester. Communicated by* Andrew Knight, *Esq. F.R.S.* Read February 9, 1815. [*Phil. Trans.* 1815, *p.* 65.]

The author's ultimate object is to ascertain the manner in which certain poisons act in destroying life; but for this purpose he found it necessary previously to determine how far the powers of the nervous and sanguiferous systems depend on each other; and though it be generally allowed that the powers of the nervous system cannot continue long after the cessation of the circulation of the blood, the converse is not so generally admitted; since there are persons who maintain that the nervous power may be wholly destroyed without impairing the vigour of the heart.

The present inquiry relates solely to this part of the subject, how far the power of the heart is influenced by the state of the nervous system; and the author designs, at some future time, to investigate experimentally, by what steps certain poisons destroy the powers of both.

M. Le Gallois maintains, that though the destruction of the brain does not impair the action of the heart, it is immediately and extremely debilitated by destruction of the cervical part of the spinal marrow. Dr. Philip, however, did not find this to be the case in his experiments, of which the first ten, performed on rabbits, relate almost exclusively to the effect of destroying the spinal marrow.

The animals were in general first rendered insensible by a blow on the occiput, after which the circulation was found to depend wholly on the continuance of respiration by artificial means, and not to be in any degree altered by subsequent removal or destruction of the spinal marrow, which was effectually done by means of a hot wire.

The next experiments related to the effects upon the heart of certain stimuli applied to the brain or spinal marrow. Spirit of wine, applied to the brain or to the upper portions of the spinal marrow, excited considerable increase of the heart's action, but not when applied to the lumbar portion. A watery solution of opium or infusion of tobacco also, occasioned a slight increase; but this was soon succeeded by more languid action of the heart, which, however, recovered its power as soon as these applications were washed off from the brain or spinal marrow.

When tincture of opium is applied to the hind legs of a frog, the animal is deprived of sensibility in less than a minute; but this effect was found not to arise from the opium, but solely from the spirit of wine in which it is dissolved, which alone has the same effect, while a watery solution of opium has no such power. But though a frog be rendered insensible by application of spirit of wine to its feet, the heart nevertheless continues to act, and its force is even increased by subsequent application of spirit of wine to the brain or spinal marrow.

The effects of opium or tobacco were also found to be the same upon frogs as they had been observed in former trials upon rabbits.

In the course of these experiments it was observed, that considerable pressure either on the brain or spinal marrow, had little or no effect on the motions of the heart; and it was further remarked, that the peristaltic motion of the intestines was not affected thereby, and indeed that it in general obeyed the same laws as those of the heart in regard to being influenced by stimuli applied to the brain or spinal marrow, but at the same time that it is not dependent on those parts for their continuance.

Since the apparent inconsistency between two facts, both well ascertained, evidently arises from some imperfection of our knowledge of the principles by which they are to be explained, the author endeavours to elucidate this subject by further experiments.

By applying strong stimuli, and repeating them, to the spinal marrow of a frog, the muscles were made to contract till their irritability was exhausted. In a second frog the nerves supplying the muscles of one leg were divided, and the irritability of its muscles was exhausted by the application of salt to the muscles themselves; and afterwards that of the other leg was exhausted by the same means, without dividing the nerves. Under these different circumstances the irritability was sooner exhausted in that limb to which the nerves remained entire. It appeared therefore, that the property of the heart, of being excitable independent of nervous influence, is common to it with other muscles; and also its property of being excited through the medium of its nerves, although it possesses the latter in a much less degree, being sparingly supplied with nerves, because its usual stimulus is immediately applied to itself, while that of the voluntary muscles is conveyed to them from the sensorium.

And it further appears to the author, that, in the same manner, the spinal marrow is capable of performing its functions independently

of the brain, yet may be influenced through the brain, the office of which is considered as purely sensorial. The separate existence of these powers is illustrated by a review of the various classes of animals, in the lowest of which we find only the muscular system; in the next above the muscular and nervous without sensorium; and in the most perfect animals we find the three vital powers combined, each having existence not immediately depending on the others, but so connected that no one can subsist long without the others, since all are supported by the same circulation, that is dependent for its continuance upon muscular action, which cannot exist without respiration, while this again depends on the nervous system for its continuance.

Although the heart of a frog retains its power long after the brain and spinal marrow are removed, nevertheless Dr. Philip found that its force may be for a time extremely impaired, by suddenly crushing the brain or spinal marrow, but it will again recover its power after the entire destruction of those parts; and corresponding effects were observed, though not so distinctly, in rabbits.

It is to this cause that the author ascribes the difference between his results and some of those of M. Le Gallois, who, instead of employing a small wire to destroy the spinal marrow, used an instrument which fitted the cavity of the spine, and consequently crushed the marrow more suddenly.

From the whole of his experiments the author concludes, that the involuntary muscles obey the same laws as those of voluntary motion; that the difference arises from their being under different stimuli; that both are liable to be stimulated through the nervous system; that they each have power independent of that system. That what has been called nervous system consists of two parts, one purely sensorial, the other conveying impressions.

That the three powers are combined in the most perfect animals. That the muscular may be destroyed through the nervous system, and the nervous through the sensorial; and though each is not strictly dependent on the others, they are so connected that no one can exist long without the others.

Experiments to ascertain the Influence of the Spinal Marrow on the Action of the Heart in Fishes. By Mr. William Clift. *Communicated by Sir* Everard Home, *Bart. V.P.R.S.* Read February 16, 1815. [*Phil. Trans.* 1815, *p.* 91.]

These experiments were undertaken by the author, in order to ascertain the truth or fallacy of M. Le Gallois' conclusion respecting the action of the heart being dependent on the spinal marrow. For since the death of quadrupeds (on which M. Le Gallois operated) is so readily produced by injury to the vital organs, it appeared to Mr. Clift that fishes would be far preferable, from their being more tenacious of life.

After two or three preliminary experiments on the duration of the

heart's action in carp, after being fully exposed by opening into the pericardium without any injury being done to the brain or spinal marrow, Mr. Clift next passed a hot wire from the tail to the occiput of a carp of the same size, so as to destroy its spinal marrow; and he found that the action of the heart was quickened for two or three beats, but then resumed the same rate of pulsation as before, although the voluntary muscles had lost their power and did not contract when a stimulus was applied to them.

After several repetitions of this experiment, with various modifications in the mode of conducting it, the author arrives at the following results:—

1st. That the muscles of the body of a carp can be thrown into powerful action four hours after the brain and heart are removed.

2dly. That those muscles lose all power as soon as the spinal marrow is destroyed.

3rdly. That by exposure of the heart to water in which the fish is allowed to swim, the action of the heart ceases sooner than in air.

4thly. That whether the heart is exposed or not, its action continues long after the brain and spinal marrow are destroyed; and still longer when the brain is removed without previous injury to its substance.

5thly. That the action of the heart is in general accelerated for a few beats by injuries to the brain or spinal marrow; but that destroying the spinal marrow after the brain has been separated renders the action of the heart slower for a few beats.

Some Experiments and Observations on the Colours used in Painting by the Ancients. By Sir Humphry Davy, *LL.D. F.R.S.* Read February 23, 1815. [*Phil. Trans.* 1815, *p.* 97.]

Beside the use which may be made of what remains of ancient paintings as models for imitation, the author has endeavoured to reap the further advantage of making us acquainted with the nature and chemical composition of their colours; for though the works of Dioscorides, Vitruvius, and Pliny contain descriptions of many substances used by the ancients as pigments, it is only by experiment that the subjects of which they speak can be identified.

The author's experiments have been made upon colours found in the baths of Titus, in the ruins called the baths of Livia, and other ruins of ancient Rome, and in the ruins of Pompeii. Some of these colours had been discovered in vases beneath the ruins of the palace of Titus, and were found to be the same as those used in various fresco paintings of the palace. In one large vase, discovered about two years since, there were found, among other colours, three different kinds of red, one approaching to orange, another dull red, and a third purplish red. The first was minium, the second and third proved to be both ochres of different tints. Another red found in various fresco paintings differed from those found in the vase, and proved to be vermilion. This substance, called by the Greeks κιννάβαρι, was known

by the name of minium to the Romans, who called our modern minium by the name of cerussa usta, in consequence of the mode of making it; which, on the authority of Pliny, is said to have been suggested by the accidental effects of a fire at the Piræeus at Athens, by which ceruse was found converted into minium.

From the description which Pliny gives of an inferior sort of vermilion, formed by calcining certain stones found in veins of lead, the author is of opinion, that the mineral thus treated must have been a natural carbonate of lead, which becomes red when burned.

Among the yellows examined by Sir Humphry Davy, were ochres of various tints, from being mixed with different quantities of chalk, and the yellow oxide of lead or massicot.

But though we have the evidence of Vitruvius that orpiment was known to the ancients, and of Pliny that a substance nearly allied to orpiment, termed Sandarach, was used by the Romans, the author has not been able to detect either of these sulphurets of arsenic in any of the ancient fresco paintings.

Among some rubbish collected in one of the chambers of the baths of Titus were several large lumps of a deep blue frit, which, upon being analysed, were found to consist of soda, silica, and oxide of copper. Upon examination of the different tints of blue observable in the paintings of the baths, as well as several blues in fragments of fresco painting from the ruins near the monument of Caius Cestius, and from excavations made at Pompeii, it appeared that they all consisted of the same blue frit, more or less diluted by admixture with carbonate of lime. There appears to the author every reason to believe this to be the colour described by Theophrastus, as discovered by an Egyptian king, and anciently manufactured at Alexandria. Vitruvius also speaks of the same colour under the name of cæruleum, made in his time at Puzzuoli, by heating together sand, flores nitri or natron, and filings of copper.

Though Pliny and Vitruvius speak of Indian blue, which appears to have been indigo, the author has not been able to discover any remains of it at this time; nor indeed of any other blue, excepting the frit before mentioned among the opake blues used by painters. But it is by no means uncommon to find among the ruins fragments of transparent blue glass, which are tinged with cobalt; and it would appear, from a passage in Theophrastus, that the Greeks considered cobalt as a species of χαλκὸς, in consequence of its property of giving this blue colour.

Among the several shades of green observable in the baths of Livia, the baths of Titus, and elsewhere, the greater part are coloured by carbonate of copper; but one of them, which approached the olive, proved to be the common green earth of Verona. It seems not improbable that some of the greens which are now found in the state of carbonate of copper may have been originally laid on as acetates; for it appears from Theophrastus that the ancients were well acquainted with verdigris.

The only trace of any thing approaching to the ancient purple

prepared from shell-fish is in a broken vase in the baths of Titus, containing a substance which at the surface has become of a cream colour, but in the interior has a lustre approaching to that of carmine. The colouring matter of this substance was found to be combustible, constituting about one thirtieth part of its weight, the remainder being a compound of siliceous, aluminous, and calcareous earths. It may, therefore, be regarded as a lake; but it would be very difficult, if not impossible, at this distance of time, to determine whether it be of animal or vegetable origin. In either case its durability, even in the interior of the mass, is a very curious circumstance, although the part exposed to the air has suffered the changes to which such colours have been too often proved to be liable, and accordingly no traces of it remain in any of the ancient fresco paintings.

All the blacks observable in the baths of Titus or elsewhere accord with the descriptions given by ancient authors, who speak of them as carbonaceous substances, obtained either as common charcoal or as soots of woods or resins.

The browns are sometimes mere oxides of iron or ochres, and sometimes mixtures of the oxides of iron and manganese; and it appears that the Romans had some knowledge of the properties peculiar to the latter substance, as Sir Humphry Davy has analysed two specimens of ancient Roman purple glass, both of which were tinged with manganese.

Among the whites of the ancient paintings, the author was unable to discover any ceruse, although it is known to have been in common use on the authority of Theophrastus, Vitruvius, and Pliny. The whites found are in general carbonate of lime, or fine white clays.

The ground to which the colours are applied in the ancient fresco paintings, is precisely such as is described by Vitruvius, powdered marble cemented by lime, highly polished and beautifully white. With regard to the mode in which their colours were applied, Vitruvius and Pliny agree as to the employment of wax in encaustic painting, which was subsequently liquefied by heat so as to give a varnish to the painting. But the author has not in any instance been able to detect the presence of wax, nor yet of any animal or vegetable gluten, in any of the fresco paintings, or even in the pot of colours found at Pompeii.

From the facts above stated, it appears that the Greek and Roman painters had the advantage over the great Italian masters, since the revival of civilization, in two of their colours, the Tyrian purple and the Egyptian azure, although the latter may easily and cheaply be imitated; for if a mixture of about fifteen parts of soda, twenty parts of powdered flint, and three parts of copper filings, be strongly heated together for about two hours, a frit is produced extremely similar in appearance and degree of fusibility to the ancient blue frit.

On the Laws which regulate the Polarization of Light by Reflection from transparent Bodies. By David Brewster, *LL.D. F.R.S. Edin. and F.S.A. Edin. In a Letter addressed to the Right Hon. Sir* Joseph Banks, *Bart. K.B. P.R.S.* Read March 16, 1815. [*Phil. Trans.* 1815, *p.* 125.]

Though Huygens, who first explained the laws that regulate the extraordinary refraction of light at a surface of Iceland spar, discovered that light thus separated has properties different from common light; and though Newton observed that light thus modified has permanent properties, with reference to the plane in which it has been refracted, and expressed this peculiarity by saying that these rays have sides according to which its subsequent refractions are regulated;—it was Malus who gave to this modification the name of polarization, a term by which he could conveniently express the various affections which such light undergoes by refraction or reflection in different directions; and could thereby most distinctly describe the various phenomena relating to his important discovery, that light may also by reflection acquire the same properties that are given by refraction through Iceland spar and other doubly refracting crystals.

Malus ascertained, that when light is incident on the surface of water at a certain angle, that portion of it which is reflected is completely polarized; and that when light is incident on the surface of other media, the angle at which complete polarization of the reflected portion takes place will be different, being greater when the reflecting substance has a higher refractive. But Malus did not succeed in detecting the rule by which the requisite angles of incidence for different bodies could be inferred from their refractive powers.

Dr. Brewster has been more successful in this inquiry; and the result of his observations now communicated is, that the angles of incidence at which the maximum of polarization is affected by all substances he has examined are such, that tangent of incidence is to radius as sine of incidence to sine of refraction; or as he expresses it, tangent of incidence is equal to the index of refraction.

And since tangent is : radius : : sine : cosine, it is evident that the angle of refraction is then complement to the angle of incidence, or their sum is a right angle; and hence the reflected ray forms a right angle with the refracted ray.

At emergence also from a dense medium, the part reflected will be more or less polarized, and the maximum of polarization will be found as before, to occur when the angles of incidence and emergence are complementary to each other, or when the reflected portion makes a right angle with the refracted ray. Hence, in polarization by a plate of glass, of which the surfaces are parallel, if the incidence on the first surface be such as to polarize the reflected ray, the portion reflected from the second surface will be polarized at the same time,—a fact which Malus had observed, but acknowledged himself unable to explain.

Dr. Brewster further observes, that it is according to the same

law that the maximum of polarization takes place in reflection from the joint surface of two media. The angle of incidence is complement to the angle of refraction, or the tangent of incidence (as the author expresses it,) is equal to the quotient of the indices of refraction of the media.

After describing in a series of propositions the various degrees in which light becomes polarized by reflection or refraction at different angles, and the number of reflections or refractions necessary to effect complete polarization at various angles remote from that which produces the maximum, the author investigates the origin of a certain quantity of unpolarized light which exists even at the maximum polarizing angle in reflection from substances of high refractive power; and he shows it to depend on the different refrangibility of differently coloured light. For when the incidence is such that the mean refrangible rays are completely polarized, it is evident that the incidence will not be such as to polarize completely either the red or the violet rays, and consequently a beam composed of these will appear as white light not polarized; and when the polarization is effected at the surface of substances of high refractive and disperaive power, this portion will form a large proportion of the whole reflected light. On the contrary, any pencil of homogeneous coloured light, though only once reflected, may be completely polarized, even at the surfaces of the densest substances, if incident at an angle correctly adapted to its refrangibility.

The author purposes, on some future occasion, to point out the laws which regulate the polarization of light under various other circumstances not noticed in the present communication.

On some Phenomena of Colours, exhibited by thin Plates. By John Knox, *Esq. Communicated by the Right Hon. Sir* Joseph Banks, *Bart. G.C.B. P.R.S.* Read April 6, 1815. [*Phil. Trans.* 1815, *p.* 161.]

It is not surprising, says the author, that neither Sir Isaac Newton, nor Dr. Herschel, nor any other writer who has followed on the same subject, have given any explanation which appears to him to be satisfactory, since they have not been in possession of the phenomena connected with this inquiry. After stating various objections which he considers as conclusive against the alternate disposition to be reflected or transmitted inherent in the rays of light, and recurring at certain equal intervals which are expressed under the name of fits by Sir Isaac Newton, Mr. Knox proceeds to describe those new phenomena which form the principal subject of his paper, and which he has been enabled to make by the assistance of the method of observing such appearances employed by Dr. Herschel. This method consists in using the shadows of some opake substance held over thin plates of glass, for the purpose of distinguishing from each other the several effects produced by different surfaces employed at the same time. If a plate of unsilvered glass be laid upon a table before

a window, and a piece of wire be held over it, there will be seen two shadows of the wire, one from each of the surfaces; if a second plate be laid upon the former, there will be three shadows, one from the uppermost surface, one from the lowest, and a third between them from the contiguous surfaces which together form but one image. If either of the surfaces in contact be slightly curved, so as to be a portion of a large sphere, then rings of colours observed by Newton are seen, and are found to arise from reflection at the surfaces of contact, as is proved by their being intercepted by the middle shadow of the wire. At the same time may also be seen other rings, occasioned by light transmitted through the surfaces in contact and reflected back to the eye from the undermost surface; but as these rings are not the subject of Mr. Knox's experiments, he painted the under surface of the lower plate black, in order to prevent their interference with his observations. Under these circumstances, a second image of the rings, formed by reflected light, is seen by means of two reflections from the parallel surfaces of the upper plate of glass; and in a favourable light further repetitions of the same rings may be seen by several successive reflections. Under these circumstances, in addition to these appearances, Mr. Knox observed certain fringes to proceed on each side from the points of intersection of a set of primary circles, with their reflected images appearing as parallel lines at right angles to the line joining their centres, and divided into two sets, coloured in opposite directions from a central line. When a piece of glass is laid upon a convex surface, the secondary set is equal to the primary, and in that case the fringes are straight lines; but when by due combination of surfaces the primary and secondary sets differ in size from each other, then these fringes assume a circular form, coloured according to the same law as the straight fringes, being divided into two classes by a middle curve, towards which the violet edges of the curves on each side are turned. To these curves, which have not been observed before, Mr. Knox gives the name of intersectionary rings.

In the same manner as one set of rings is produced by the intersection of primary and secondary reflected circles with each other, they may also be produced by the intersection of other sets, either of transmitted or reflected rings, and may be rendered numerous by a number of surfaces of various forms; or, on the contrary, may be exhibited in their most simple state by the intersection of primary fringes with each other. For if two slips of plane glass be applied to each other at a small angle, the fringes of colour then appear as straight bars of prismatic colours parallel to each other. And if a third plate of glass be placed upon the uppermost, with a slight inclination situated transversely to the former, the bands thus produced are at right angles to the preceding; and by their intersections present a set of intersectionary fringes parallel to each other, and bisecting the angle between the primaries from which they originate. These fringes, as in the former cases, are divided by a central band into two sets, of which the colours are oppositely placed, and on this

account the author calls them *binary*, a term which applies to the numerous class of phenomena which he has observed by a great variety of combinations. He reckons as many as six kinds of rectilinear bands produced in his experiments which have not been noticed by any other writer.

In order to ascertain what effect the presence of air might have on these phenomena, the author repeated some of his experiments in vacuo, and found that the removal of the air had no perceptible effect.

Even the interposition of water between the surfaces appears to him to diminish but little the brilliancy of the colours. Nitric acid has more effect; and in fluids of greater density, as olive oil, the whole class of phenomena disappear.

It appears somewhat strange, says Mr. Knox, that Newton should have attributed the coloured rings to a plate of air and to supposititious fits of easy reflection and refraction, when a cause more obvious was at hand; namely, the interference of the reflecting and refracting strata diffused over the contiguous surfaces: for it may be supposed, that when a ray passing out of glass into air is interrupted and receives a new impulse by the influence of a second refracting medium, these contrary impulses may be repeated many times, and by repeated vibration may affect the rays according to their different refrangibility, so as to separate them into differently coloured spectra. He therefore thinks it highly probable, that by this compound action and reaction between the strata and light, and between the rays of light themselves, all the various phenomena are produced, although from their extreme minuteness an accurate knowledge of the mode of operation is not to be expected.

Some farther Observations on the Current that often prevails to the Westward of the Scilly Islands. By James Rennell, *Esq. F.R.S.* Read April 13, 1815. [*Phil. Trans.* 1815, *p.* 182.]

In the course of twenty-one years that have elapsed since the author's original communication on this subject was published in our Transactions, he has collected many new instances of the effects of the current, tending to confirm the general observations respecting its course from Cape Finisterre to Scilly, and affording clearer proof of the strength of the stream than any evidence that he could adduce on the former occasion. The first fact relates to its commencement in an easterly direction, toward Cape Finisterre, from a distance of at least fifty-three leagues, in the instance of the Earl Cornwallis Indiaman, which drifted in that direction at the rate of twenty-six miles per day.

In the second instance, a bottle thrown out by a Danish navigator was carried in a direction E. by S. to Cape Ortegal, a distance of sixty-four leagues.

A third fact was communicated to the author by Admiral Knight.

who found a current at the rate of one mile per hour, setting nearly along shore on the north coast of Spain.

With respect to the progress of the current which thus evidently sets along the southern side of the Bay of Biscay, the author has not been able to procure any further evidence directly showing its continuance round the Bay; but he observes that the soundings evidently show that the mud of the Garonne, Charante, Loire, &c., on the west coast of France, is all carried to the northward; and that the openings of these rivers evidently point in that direction in consequence of the current that sets across their mouths.

With regard to the further progress of the same current to the northward along the coast of France, direct evidence is again obtained in the instance of H. M. S. Russel, which was carried seventy miles to the N.W. by an unexpected current in that direction.

In addition to these facts, which are of comparatively recent occurrence, the author finds that the generally observed and well-known flow of the tides on the west of Scilly, cannot well be accounted for but by the supposition of a current setting in from the southward, which causes the flood tide to run nine hours northward, while the ebb in the opposite direction lasts only three hours.

Major Rennell has also recourse for evidence to two publications which have but recently come to his knowledge, though printed so long since as 1733 and 1757. In Joshua Kelly's Treatise on Navigation, of the former date, an instance is related of a West Indiaman drifted, during two days of dead calm, forty-six miles northward, across the mouth of the British Channel.

Captain Mead also, in relating the case of the ship Hope of Liverpool, in 1735. mentions her having been drifted forty-eight miles to the northward of her expected course, by the same current which he represents as an indraught into St. George's Channel; and says it was well known to the Bristol men in making for their Channel, and that accordingly they made allowance in their bearing of 4° or 5° for that indraught.

The author takes this occasion to notice another current, setting to the north-eastward across St. George's Channel into Cardigan Bay, which sometimes endangers the safety of vessels in their passage from the Land's End to Dublin.

This may partly arise from the tail of the Scilly current having an easterly set, in consequence of meeting the S.E. coast of Ireland; but in the author's estimation is more likely to originate from a cause similar to that of the Scilly current itself, in consequence of the south-westerly winds carrying a heavy sea along that coast from Cape Clear to Carnsore Point.

Some Experiments on a Solid Compound of Iodine and Oxygen, and on its Chemical Agencies. *By Sir* Humphry Davy, *LL.D. F.R.S.* Read April 20, 1815. [*Phil. Trans.* 1815, *p.* 203.]

The author having observed that when a compound of iodine and chlorine was poured into an alkaline solution, there occurred a precipitate of iodine combined with oxygen, inferred that iodine would in all probability decompose the gaseous compound of oxygen and chlorine; and upon trial found this presumption confirmed. For when iodine is exposed to euchlorine at the common temperatures of the atmosphere, there is an immediate action, and the formation of two compounds, an orange-coloured liquid consisting of chlorine and iodine, and a white powder composed of iodine and oxygen.

By the application of a gentle heat, the former is made to rise in vapour, and the latter then remains as a semi-transparent white solid. It has no smell, but a strong astringent sour taste. Its specific gravity is such that it sinks in strong sulphuric acid.

By heat, rather below the temperature of boiling oil, this compound is separated into its two constituents, iodine, which crystallizes on the sides of the vessel, and a gas which is found to be pure oxygen. The proportions of these products are such, that the author conceives it to consist of one portion of iodine with five doses of oxygen.

This compound has such affinity for water that it slowly deliquesces in a moist atmosphere, but remains unaltered when the atmosphere is dry. When dissolved in water, it first reddens and then destroys vegetable blues. By distillation the water rises in vapour, and by moderate heat leaves the solid substance unaltered.

The solution acts upon all metals, even upon gold and platina, and decomposes many metallic solutions, occasioning insoluble precipitates from solutions of lead or mercury, which are oxides of those metals.

It also forms compounds with all the earths that have been tried, (some of which are nearly insoluble in water,) and with alkalies it forms the same compounds that have formerly been made by other means.

This oxide also combines with acids, forming compounds which crystallize. These are intensely acid to the taste, they redden vegetable blues, and act strongly on all metals. The effects of heat upon them are various, according to the different nature of the acid with which they are combined. The nitrate and sulphate may be sublimed unaltered, but are liable to partial decomposition if too suddenly heated.

The oxalate is immediately and entirely decomposed by a gentle heat, and yields iodine and carbonic acid.

Since those acids which are obtained by the decomposition of such compounds are found to be in the state of hydrates, it is evident that water is a constituent of the crystalline products.

To the compounds of iodine, oxygen, and bases, the author formerly gave the name of oxyiodes; to the new compound of iodine

and oxygen, he now gives the name of oxyiodine; and to its compounds with water, oxyiodic acid. To this same compound M. Gay-Lussac has given the name of iodic acid, and for the salts he uses the generic term iodates; but to this the author objects, that hydriodic and chloriodic acids may each be as well entitled to the appellation of iodic acids as a generic name, and that the termination in *at* places those bodies, which he calls oxyiodes, in the common class of neutral salts, from which they differ in many respects; while the term oxyiodes expresses more definitely the nature of a combination, which has the closest analogy with the bodies termed hyperoxymuriates.

On the Action of Acids on the Salts usually called Hyperoxymuriates, and on the Gases produced from them. By Sir Humphry Davy, *LL.D. F.R.S.* Read May 4, 1815. [*Phil. Trans.* 1815, *p.* 214.]

M. Gay-Lussac having obtained, by the action of sulphuric acid on hyperoxymuriate of barytes, a peculiar compound, to which he gave the name of chloric acid, the author was induced to examine the action of this and other acids on the hyperoxymuriate of potash, and after various attempts, found the following process with sulphuric acid to be the best. A small quantity, not exceeding fifty or sixty grains, of the hyperoxymuriate are to be mixed with a small quantity of the acid in its concentrated state, and to be rubbed together by means of a spatula of platina till incorporated into a solid mass of a bright orange colour. This mass having been introduced into a small retort, is to be then warmed by immersion in water gradually heated, but kept below the boiling point. As the heat rises, an elastic fluid is emitted of a bright yellowish green colour. This gas may be received over mercury, on which it has no action; but it is rapidly absorbed by water. Its smell is aromatic, without any smell of chlorine. It destroys vegetable blues, without previously reddening them. By a temperature of 212° it explodes with more violence than euchlorine, expanding more, and producing more light. After the explosion the volume is found increased in the proportion of 2 to 3; two parts of the product being oxygen, and the remainder chlorine.

Phosphorus introduced into this gas occasions an explosion, and burns in the liberated gases with its usual brilliancy; but other combustible bodies have no action on the gas.

Water saturated with the gas is of a deep yellow colour; it does not taste sour, but astringent and corrosive, leaving a lasting and disagreeable impression on the tongue.

It appears to the author not impossible, that the gas to which he formerly gave the name of euchlorine, may be a mixture of the new gas with chlorine; and indeed the action of water upon euchlorine favours this idea, since it acquires the same colour from it, and leaves a residuum of chlorine; but, on the contrary, the circumstance that Dutch foil has no action upon euchlorine, seems to show that it contains no free chlorine merely intermixed, but that the whole is chemically combined.

The saturated solution of the new gas in water, when mixed with alkaline solutions, does not immediately lose its colour, or neutralise the alkalies, but after a time the hyperoxymuriates are formed, and the colour disappears.

In consequence of the doubt which now occurs concerning the true nature of euchlorine, the author declines giving a name to the present compound, till he can have an opportunity of making some new experiments on that subject.

Further analytical Experiments relative to the Constitution of the prussic, of the ferruretted chyazic, and of the sulphuretted chyazic Acids, and to that of their Salts; together with the Application of the Atomic Theory to the Analyses of those Bodies. *By* Robert Porrett, *jun. Esq.* *Communicated by* W. H. Wollaston, *M.D. Sec. R.S.* Read May 11, 1815. [*Phil. Trans.* 1815, *p.* 220.]

In a former paper the author endeavoured to show that prussic acid would combine with black oxide of iron, or with sulphur, and form with them peculiar acids, to which he gave the names of ferruretted and sulphuretted chyazic acids; and he examined in what proportion the elements of these new acids are combined, as well as the proportions in which the acids unite to different saline bases.

Mr. Porrett's present object is to add the results of two new analyses, made with great care, and to correct those inaccuracies of experiment, which are at all times unavoidable, by the assistance of the theory of Dalton respecting the relative weights of atoms, and of Berzelius respecting multiple doses of oxygen.

The first compound here examined, is prussiate of mercury. Ten grains of this salt were decomposed by hydrosulphuret of soda, and yielded 9·3 of black sulphuret of mercury. The residual liquor being treated with sulphate of copper, yielded 9·7 of sulphuretted chyazate of copper, which by former analyses is known to contain 1·38 prussic acid.

In order to determine the quantity of red oxide of mercury indicated by the weight of black sulphuret obtained, 25 grains of corrosive sublimate were decomposed by hydrosulphuret of potash, and yielded 21·5 grains; and since the quantity of red oxide contained in the sublimate is known to be $79\frac{1}{4}$ per cent., it is inferred that 9·3 black sulphuret are equivalent to 8·62 red oxide; and hence that 100 grains of prussiate of mercury consist of 13·8 prussic acid, and 86·2 red oxide of mercury.

For the purpose of determining the constitution of prussic acid, Mr. Porrett effected its decomposition by mixing prussiate of mercury with red oxide of mercury, in such proportion, that by the assistance of heat, the whole of the prussic acid might be converted into carbonic acid, azote, and water. By a number of trials he found that this was not completely effected till the quantity of red oxide added amounted to five times the quantity contained in the prussiate. In the decomposition of prussiate of mercury alone by heat, it is only

one sixth part of the prussic acid, which is decomposed by the quantity of oxygen in the oxide present as a constituent of that salt, and hence five more equal quantities are requisite to effect the complete decomposition of the whole. In all cases it is observable that the quantity of azote produced is exactly equal in volume to the quantity of prussic acid gas decomposed, and the quantity of carbonic acid exactly the double of the same measure. Together with these is produced a quantity of water, containing twice as much oxygen as is contained in the carbonic acid.

The author takes pains to describe, with much precision, the precautions which he found it expedient to employ for effecting the entire decomposition of the prussic acid, the mode of preparing the red oxide, of grinding the materials, of charging the tube that he employs as a retort, of applying the heat to the several parts in succession, and of receiving and examining the products.

The results of this analysis of prussic acid, show that

100 grains consist of 34·8 carbon.
40·7 azote.
24·5 hydrogen.

In a Table which follows, the author exhibits, at one view, the results of his analysis of prussic acid, and of ten different compounds into which it enters; and at the same time a comparative statement of those proportions which may be supposed more near approximations to the truth, from theoretic considerations of the number of atoms contained in each of the salts under examination.

On the Nature and Combinations of a newly discovered vegetable Acid; with Observations on the Malic Acid, and Suggestions on the State in which Acids may have previously existed in Vegetables. By M. Donovan, *Esq. Communicated by* William Hyde Wollaston, *M.D. Sec. R.S.* Read June 1, 1815. [*Phil. Trans.* 1815, *p.* 231.]

The acid here noticed by the author being obtained in greatest quantity from the fruit of the *Sorbus aucuparia*, is denominated by him sorbic acid, in order to distinguish it from other known vegetable acids. To prepare it, he presses the ripe fruit, previously bruised, in a linen bag, and thereby obtains nearly half its weight of juice. With this juice he mixes a solution of acetate of lead, and obtains a precipitate of sorbate of lead, which requires to be frequently washed with cold water. The purified powder is then boiled in a large quantity of water, which dissolves a part as a super-sorbate, leaving undissolved a sub-sorbate. The liquor being filtered and suffered to cool, deposits brilliant crystals of purified sorbate of lead.

To the crystals thus obtained he adds a quantity of dilute sulphuric acid, sufficient to separate nearly the whole of the lead; and having then separated the remainder by a current of sulphuretted hydrogen gas, he obtains the acid in a state of purity.

The acid to which this bears the nearest resemblance, is the malic; and indeed these two acids appear to the author to have been con-

founded even by Scheele, for apples contain a portion of sorbic as well as of malic acid; but that these acids are different, Mr. Donovan proves by decomposing malate of lead by sorbic acid. For if water be boiled on malate of lead, no crystals are to be obtained on cooling the liquor; but when the malate is boiled in sorbic acid, the malate is decomposed; and the liquor, when cooled, deposits the peculiarly brilliant crystals of sorbate of lead.

In order to be well assured of the difference between these acids, Mr. Donovan compared his acid with as many as seven different specimens of malic acid obtained from different sources, and was confirmed in the opinion that they are essentially different, by comparison of various neutral salts obtained from each; those containing sorbic acid being in general to be procured in permanent crystals, while those from malic acid yield merely deliquescent residua when reduced to dryness.

The author remarks, that the purest malic acid is that prepared from the *Sempervivum tectorum*, which, according to the observation of Vauquelin, appears to be free from every other acid; while the juice of apples, unless they be taken very young, appears constantly to contain a portion of sorbic acid.

This paper concludes with conjectures respecting the progressive changes of vegetable products, and possible conversion of bitter principle into malic acid, sorbic acid, and oxalic acid; but the author is fully sensible that little reliance can be placed on such speculations.

On the Structure of the Organs of Respiration in Animals which appear to hold an intermediate Place between those of the Class Pisces and the Class Vermes, and in two Genera of the last-mentioned Class. By Sir Everard Home, *Bart. V.P.R.S.* Read June 1, 1815. [*Phil. Trans.* 1815, *p.* 256.]

The genera of animals here enumerated by the author, are the Lamprey, Myxine, an animal between the Lamprey and the Myxine, the *Aphrodita aculeata*, and the Leech.

In the Lamprey, the organs of respiration consist of separate oval bags, that have seven openings on each side of the neck, for receiving and emitting the water which they breathe by means of a cartilaginous thorax surrounding the bags. In the Lampern, which is of the same genus, the structure is very similar, but the cartilages of its thorax are weaker.

In an animal brought from the South Sea by Sir Joseph Banks, there are also the same number of external openings, and the same number of bags; but there is no cartilaginous thorax, and hence the author is induced to consider the animal intermediate between the Lamprey and Myxine, which it resembles in having teeth, and in having a mesentery to its intestines.

In the Myxine, there are only two orifices on the under surface of the neck; but these branch internally to six separate bags on each side.

these occasions have been from 300 to 800 times; sometimes one, and sometimes another being used, according to the states of the air or moon, or other circumstances.

The observations here recorded, were made principally from the year 1787 to 1798 inclusive, and they are given in the order in which they occurred with all the phenomena noticed each time, and notes of correction added from subsequent observations respecting stars mistaken for satellites, or satellites wrongly numbered. For Dr. Herschel always added a configuration to his descriptions, in order to avoid mistakes, and in general made, by previous estimate, a sketch of the places where known satellites might be expected; some misconceptions were unavoidable, in consequence of the interruptions to his observations from cloudy or moon-light nights, as well as from small stars that unexpectedly interfered.

From all the observations thus made, and from accurate measures taken by different micrometers, the author infers the nodes of the two first satellites to have nearly the same longitude of 165½°; that their inclination is also the same, 78° 58′; and the motion of both, from their ascending node to the greatest elongation, retrograde. The periodic time of the first is found to be $8^d\ 16^h\ 56^m\ 5^s$; and that of the second $13^d\ 11^h\ 8^m\ 19^s$; their distances at greatest elongation 36″ and 48″ respectively.

To these determinations respecting the orbits of the satellites, Dr. Herschel adds his estimate of the distances to which they must move from the body of the planet before they become visible by his 20-feet telescope. The first requires to be at more than half its greatest elongation. The second also becomes invisible when it is within half its greatest elongation. If there be an interior satellite, as large as the first, he imagines it would be visible through so small a part of its orbit, as not to be seen for two nights together.

Notwithstanding this difficulty, the author thinks he has seen an interior satellite. And with regard to exterior satellites, though nothing has been correctly ascertained, the number and positions of different objects recorded is such, that the author enumerates as far as a supposed sixth satellite.

An Account of some Experiments with a large Voltaic Battery. *By* J. G. Children, *Esq. F.R.S.* Read June 15, 1815. [*Phil. Trans.* 1815, *p.* 363.]

The battery with which these experiments were made, has 21 cells, each containing about 40 gallons of water, to which was added a mixture of nitric and sulphuric acids, at various intervals, beginning with $\frac{1}{30}$th, and ending with $\frac{1}{10}$th part of the water. Into each cell was immersed a zinc plate, with a pair of copper plates, one on each side, but connected together at the bottom, and also duly connected to the zinc in the cell adjacent. Each plate had 16 square feet of surface.

The first series of experiments were on the comparative liability of

different metals to be ignited by the power of this battery, by joining equal lengths of any two wires in the same line, and making the circuit through both thus connected. In the comparison of platina and iron, which of all metals are most easily ignited, the difference was so little, that their comparative ignition altered during the experiment in consequence of apparent difference in their capacities, as well as conducting powers. Of other metals, gold and copper were nearly equal, and far less easily ignited than the former. After them stood zinc; and last of all silver. Mr. Children observes, that the order of these metals, as conductors of electricity, nearly follows that of their powers to conduct heat.

When this battery was excited to its utmost, it ignited 5½ feet of platina wire one ninth of an inch in diameter.

A bar of platina, one sixth of an inch square and 2¼ inches long, was heated red, and fused at each end.

A round rod, one fourth of an inch in diameter and 2½ inches long, was heated bright red throughout.

Oxide of tungsten was partially reduced.

Oxide of tantalum was partly fused, and of a reddish brown colour.

Oxides of uranium and titanium were fused, but not reduced.

Oxide of molybdena was fused and reduced.

Oxide of iridium, with osmium, was fused to a globule.

Pure iridium was fused into an imperfect globule, having specific gravity 18·68.

Of earthy bodies, ruby and sapphire were not fused.

Blue spinel ran to a slag.

Magnesia was agglutinated.

Quartz was not affected.

This opportunity was taken by Mr. Pepys of attempting the conversion of iron into steel by union with diamond, which appeared to have been accomplished. A wire of good soft iron, having been slit longitudinally with a fine saw, the slit was filled with diamond powder, and the whole having then been wrapped round with a piece of muscovy talc, was bound together with a fine iron wire. Although the wire thus prepared was by no means intensely ignited, and for no longer a time than six minutes, no part of the diamond powder was to be found after the experiment, and the iron was found converted into a sort of blistered steel; for it was hardened by quenching while hot, so as to resist the file, and to scratch glass with facility.

Beside the experiments above made with three plates connected in succession as a powerful battery by the action of acids on their surfaces, a trial was made whether at the moment of contact between very large metallic surfaces, any degree of ignition could be rendered visible. All the zinc plates were connected together as one zinc plate, and also all the copper plates connected as one copper plate. A communication was then made between the two sets of plates thus connected, but *not immersed* in a fluid, and all the electricity supposed to move in consequence of the contact, was made to pass through a wire of platina $\frac{1}{300}$th of an inch in diameter, and about

$\frac{1}{70}$th of an inch in length; but there was no appearance of ignition, although the same wire may be instantly ignited by a single pair of one inch plates immersed in a weak acid.

On the dispersive Power of the Atmosphere, and its Effect on astronomical Observations. By Stephen Lee, *Clerk and Librarian to the Royal Society. Communicated by* W. H. Wollaston, *M.D. Sec. R.S.* Read June 15, 1815. [*Phil. Trans.* 1815, *p.* 375.]

Although the appearances of colour given to low stars by atmospheric refraction be very well known, the comparative degree of refrangibility of the differently coloured rays does not appear to have attracted attention in proportion to its important effects on delicate astronomical observations.

The author endeavours to point out some of the principal errors that may arise from making allowance for mean refraction without due discrimination of the kinds of colour observed. It is evident that stars of different colours will require different corrections in observations of their altitudes.

The apparent altitude of the sun will also vary, according to the coloured glass employed in viewing its disc. For since there must, in fact, be several images of the limb observed at small distances from each other, it becomes a matter of choice which of them shall be selected by the kind of glass used; and it is possible, that to their cause may be ascribed the discordance which exists between the observations of the solstices, and possibly some disagreement between different observers.

Mr. Lee also suggests, from this source, an explanation of the apparent projection of Aldebaran and other red stars upon the surface of the moon,—a phenomenon that has been frequently noticed, but not yet understood. For if Aldebaran be nearly in contact with the upper limb, since the white light of the moon will be elevated more by refraction, it is evident that the stars may thus be made to appear within her disc a few seconds before or after contact.

The author refers to a great number of observations that he has made on Mars, Venus, and fixed stars; from which he infers the quantity of dispersion of light to be between one sixtieth and one seventieth of the total atmospheric refraction.

He also adds several remarks on certain alterations in the mode of making astronomical observations, by which the results deduced may have been affected, especially with reference to those of Dr. Bradley; noticing particularly the period when Hadley's sextant came into general use, and with it the employment of glasses variously coloured, which were soon applied to other instruments.

Mr. Lee concludes with suggesting such precautions as may lead to a more correct knowledge of atmospheric refraction, hoping that the subject may be pursued by astronomers more favourably situated than himself for such an investigation.

Determination of the North Polar Distances and proper Motion of Thirty fixed Stars. By John Pond, *Esq. Astronomer Royal, F.R.S.* Read June 15, 1815. [*Phil. Trans.* 1815, *p.* 384.]

In the former catalogue which the Astronomer Royal gave in 1813, he estimated the probable extent of error at not more than one fourth of a second; and his present catalogue, which may be presumed to be improved by a greater number of observations, confirms the justness of that estimate; since the greatest difference observable is not more than two tenths of a second.

A comparative catalogue is also given of the places of the same stars in 1756, as deduced from the observations of Dr. Bradley; and thence is added a column of annual proper motions for each of the stars in the collection.

An Essay towards the Calculus of Functions. By C. Babbage, *Esq. Communicated by* W. H. Wollaston, *M.D. Sec. R.S.* Read June 15, 1815. [*Phil. Trans.* 1815, *p.* 389.]

In the same manner as an exponent expresses one operation on quantity, namely, the multiplication of it by itself a certain number of times, or raising it to the power expressed by that exponent, so the term Function, which has been introduced into modern analysis, is intended to express generally the results of all the various operations that can be performed upon quantity. Many of these operations consist of two parts, the *direct* and the *inverse*. To extract a root is the inverse, with reference to that of raising any number to a higher power. So the *integral* is the inverse of the differential calculus; and the same observation applies to finite differences. In all these cases the inverse method is by far the more difficult of the two.

The author's object in the present essay is to consider the inverse method with respect to functions, and, if possible, to determine the value of an unknown function by means of any functional equation given, instead of discovering, as in the direct method, the value of a quantity from an equation in which the function is known. But the mode in which the author pursues his inquiry, of course could not admit of being publicly read.

Some additional Experiments and Observations on the Relation which subsists between the Nervous and Sanguiferous Systems. By A. P. Wilson Philip, *Physician in Worcester. Communicated by* Thomas Andrew Knight, *Esq. F.R.S.* Read June 15, 1815. [*Phil. Trans.* 1815, *p.* 424.]

This paper comprises a series of very numerous experiments on the effects of various stimuli applied to the brain and nerves of rabbits and frogs, in exciting the voluntary muscles, the heart, and the blood-vessels; from which the author infers,—

That the laws which regulate their effects on the muscles of voluntary and involuntary motion are different.

That both mechanical and chemical stimuli applied to any part of the nervous system tend to increase the action of the heart.

That neither mechanical nor chemical stimuli applied excite the muscles of voluntary motion, unless they are applied near the origin of the spinal marrow.

That mechanical stimuli have more effect than chemical on voluntary muscles, but the reverse with respect to those of involuntary motion.

That all stimuli continue to affect the heart long after they have failed to excite the muscles of voluntary motion.

That the motions thus excited in voluntary muscles are irregular, but those of the heart more regular.

That the former occur chiefly at the first moment of application, but those of the latter as long as the stimulus is applied.

That the former depend on intensity of the stimulus, the latter on the extent of surface to which it is applied.

That the power of the blood-vessels, like that of the heart, is independent of the nervous system, though they may be influenced through that system, as the heart is.

That the actions thus excited are regular, as those of the heart, and that their power, like that of the heart, may be destroyed through the nervous system.

On the Fire-damp of Coal Mines, and on Methods of lighting the Mines so as to prevent its Explosion. By Sir H. Davy, *LL.D. F.R.S. V.P.R.I.* Read November 9, 1815. [*Phil. Trans.* 1816, *p.* 1.]

The great sources of fire-damp in coal mines are blowers or fissures from which currents of this inflammable gas issue in considerable quantities and for a long series of years; but there is also a certain quantity produced by the workings. The author was informed by Mr. Hodgson, that if a cask be filled with a quantity of recently pounded coal, and a small aperture be made in it, inflammable gas will issue from the aperture.

In several specimens of fire-damp which the author has analysed, the inflammable part was the same in all; in some instances mixed with a small quantity of common air, in others with azote and carbonic acid. The purest contained only $\frac{1}{15}$th of atmospheric air. One measure of this gas required nearly two measures of oxygen for its combustion, and formed nearly one measure of carbonic acid.

Sulphur heated in this gas decomposed it, forming sulphuretted hydrogen, and precipitating charcoal.

This gas, when mixed with chlorine, does not combine by exposure to light; so that it appears to contain neither olefiant gas nor hydrogen, and seems to be the same as the inflammable gas of marshes, or pure carburetted hydrogen.

When this gas is mixed with atmospheric air, it inflames by a lighted taper; but unless the quantities are duly proportioned, the inflammation is too slow to be accompanied with sound. The combustion appeared to be most sudden when there were about 7 or 8 parts of air to 1 of the gas. When 1 of the gas is mixed with 15 or 16 of air, the candle burns with an enlarged flame, which does not extend to the whole quantity, and the same phenomenon occurs when the gas constitutes no more than $\frac{1}{15}$th part of the mixture.

The author next made trial of the degree of heat necessary for the combustion of this gas, which in fact is difficult to ignite, for even the electric spark will not explode a mixture containing 5 parts of air to 1 of the gas; but when the air is increased as far as 6 to 1, or nearer to the most explosive proportion, then it does take fire by the electric spark. Even the most explosive mixture will not take fire from well burned charcoal, nor by iron heated to any degree short of actual combustion. But on the contrary, hydrogen whether simple or sulphuretted, olefiant gas, or carbonic oxide, may each be set on fire very readily by all these means; and hence the gas procured by distillation from coal is liable to be fired by ignited iron or charcoal, in consequence of the olefiant gas which it contains along with the carburetted hydrogen in the first stage of the process of distillation, and on account of the carbonic oxide and hydrogen which it contains in the latter stages of its production.

Since the fire-damp of coal mines is free from these admixtures, it might be possible to light a mine by means of a charcoal fire, if free from flame, which is necessary for the ignition of the gas.

The method, however, which the author proposes, is not dependent on this property, but upon the supply of air to a lamp employed for that purpose being limited, in such a manner that the lamp will be extinguished by impurity of the air before an explosion can take place.

If a lighted lantern, in the bottom of which there is only a small aperture just sufficient for its supply, be inserted in a mixture of common air, with about $\frac{1}{15}$th of fire-damp, the flame first appears feeble, next enlarges, and then is uniformly extinguished. In the lantern here proposed for coal mines, a chimney is added of such length as to prevent any communication from the enlarged flame upwards with the external air; and the opening below is designed to be at a fixed distance from the flame, by using oil lamps instead of candles. As an additional security in some cases, the author conceives it may be useful to add valves at the apertures, which may be made to shut by a thermometric spring when the heat is increased by enlargement of the flame.

Beside the foregoing, which the author terms the safe lantern, he has constructed two others on the same principle with that proposed by Dr. Clanny, and described in a late volume of our Transactions: but as that of Dr. Clanny has been found objectionable, not only on account of manual labour and constant attention necessary in working the bellows, but also on account of its size, those now proposed

are of smaller dimensions, one with a pair of small bellows, and t other with a syringe; but both of these, like the steel mill, require person to work them constantly, and will probably be superseded l the simple construction first described.

An Account of an Invention for giving Light in explosive Mixtures Fire-damp in Coal Mines, by consuming the Fire-damp. By S Humphry Davy, *LL.D. F.R.S. V.P.R.I.* Read January 11, 181 [*Phil. Trans.* 1816, *p.* 23.]

This contrivance consists in covering the flame of any lamp or ca dle with a wire sieve, the apertures of which may be as large as $\frac{1}{10}$ of an inch square.

When a cylinder of wire gauze, covered at top with the same gauz is closely fitted to a lamp, and surrounds its flame so that there is i aperture but those of the gauze, if the little lantern so construct be introduced into the most explosive mixtures of carburetted hydr gen and air, the cylinder becomes filled with a bright flame at its in terior surface, which continues to burn as long as the mixture remai explosive.

When the carburetted hydrogen constitutes no more than $\frac{1}{12}$th the mixture, the flame of the wick continues to burn surrounded b the flame of the fire-damp; but when the proportion is as 1 to 7, th flame of the wick disappears.

When the apertures of the gauze are of the largest size, the flam is the most brilliant, and the wire of which it consists becomes ignite but still without occasioning explosion of the mixture external to th lamp.

Similar experiments were also made with hydrogen (not carbu retted), and with the same results. But in this case the gauze wa of the finest kind, with 6400 apertures in the square inch; and sinc the thickness of the wire was $\frac{1}{160}$th of an inch, the apertures them selves were about $\frac{1}{160}$th of an inch square. With gauze of this con struction a flame may be introduced even into a mixture of hydroge and oxygen, and burn this mixture at the inner surface of the lan tern, without communicating its flame to the mixture externally even though the wires become intensely red hot.

After such trials of the security of this little apparatus, the autho has no hesitation in recommending it for adoption by the collier, wh will require for his security nothing more than a little wire cage t surround the flame of his lamp, and thus, at the cost of a few pence will procure a light as long as there is a sufficient supply of fire-damp without any further expense.

On the Developement of Exponential Functions; together with several new Theorems relating to finite Differences. By John Frederick W. Herschel, *Esq. F.R.S.* Read December 14, 1815. [*Phil. Trans.* 1816, *p.* 25.]

The subject here considered by Mr. Herschel relates to the celebrated theorems of Lagrange, expressing the connection between simple exponential indices and those of differentiation and of integration.

Since the theorems have been demonstrated by various subsequent analysts, as by Laplace, by Arbogast, and by Dr. Brinkley, the author takes them for granted; but observes that in their original form they are but abridged expressions of their meaning; and that in order to become practically useful, their exponential functions require further development.

And though this part of the subject has been treated with great ability by Dr. Brinkley, who has deduced formulæ respecting the first of the two theorems far more simple than could have been expected from the complex nature of the subject; yet since his method, when applied to the second more general theorem, would lead to details of extreme complexity, Mr. Herschel has taken a different view of the subject; and beginning with the more general theorem has arrived at a general formula, which he believes to have been hitherto wholly unnoticed, and which, when applied to certain particular cases treated of by Dr. Brinkley, affords precisely the same results.

But the mode in which this subject is treated, or even the results, were not of a nature to admit of being read in public.

On new Properties of Heat, as exhibited in its Propagation along Plates of Glass. By David Brewster, *LL.D. F.R.S. Lond. and Edinb. In a Letter addressed to the Right Hon. Sir* Joseph Banks, *Bart. G.C.B. P.R.S.* Read January 11, 1816. [*Phil. Trans.* 1816, *p.* 46.]

In a paper communicated to the Society in May 1814, Dr. Brewster observed that glass, when raised to a high temperature, had the property of depolarizing light, and in this respect resembled crystallized substances; but he did not at that time succeed in tracing a resemblance in other points, which he left for future investigation.

On resuming this inquiry in the present paper, the subject is divided into two parts; in the former of which he describes the transient effects exhibited during the propagation of heat along plates of glass, whether received from adjacent bodies or communicated to them; and in the latter he describes the permanent optical properties produced in glass by being suddenly and partially cooled when red hot.

The phenomena here noticed depend on the total or partial depolarization of light, previously polarized by reflection at a certain angle from a surface of black glass. It is well known that when a ray of light thus polarized in the plane of primitive incidence is incident upon a second surface of black glass at the same specific angle,

if the second plane of incidence be at right angles to the former, then no part of that ray will be reflected by the second surface; and an eye rightly placed for witnessing the effects, will perceive in the exact specific direction a central spot of absolute blackness, surrounded also by a dark space of some extent, from which less or more of light is reflected, in proportion to the increase of distance from the central line of no reflection. The light, however, which has thus been polarized, may be wholly, or in part, depolarized by the interposition of many crystallized bodies, the degree of depolarization being dependent on the more or less exact position of a certain neutral axis or plane of their crystalline texture with the plane of primitive polarization. When these planes are perfectly coincident, the light remains polarized in that plane, and a black line appears in that direction; but adjacent to it, on each side, are seen a series of colours, which depend partly upon their proximity to the central black line, and partly upon the thickness of the depolarizing body, the succession of colours being exactly the same as those observed by Newton in thin plates, but variously modified in their forms, according to the nature of the crystalline substance interposed, and according to the position of its axis.

The optical effects of heated glass, as now observed by Dr. Brewster, are precisely of the same kind, and are now found to depend not upon the simple circumstance of temperature of the entire plate, as he originally supposed, but upon the progressive differences of temperature in different parts of the plate, arising from contact or proximity to a plate of hot or cold iron, or from the cooling power of the surrounding atmosphere. And in the same manner as the several tints of colour produced by crystallized bodies, have been shown by M. Biot to depend on a series of thicknesses proportional to those in Newton's scale for thin plates; so with respect to heated glass, Dr. Brewster observes, that a corresponding arithmetical progression is observable for the same tints, whether the thicknesses compared be those of single plates, or the aggregate thickness of several combined.

From these phenomena, Dr. Brewster infers the production of what he calls a crystalline structure in the glass during its contact with heated iron; but observing the existence of an opposite structure in the middle of the glass, and again the same structure at its remote extremity apparently beyond the reach of sensible heat, he says there is nothing analogous but in the perplexing phenomena of magnetic and electric polarity.

In the prosecution of these experiments, the author varies indefinitely the forms and dimensions of his plates of glass, and with them the forms of the fringes produced; but it would be next to impossible to convey any correct idea of the various appearances without assistance from drawings; neither indeed could Dr. Brewster himself have observed the phenomena with sufficient precision in their fluctuating state as arising from the temporary communication of heat, had he not found means to render the same properties permanent.

by a method which forms the subject of the second section of this paper.

When a plate of glass is brought to a full red heat, and is then cooled by placing its edge on a bar of cold iron, the same fringes of colour are developed during cooling as by placing cold glass upon hot iron; and in this case the glass retains the property given to it even after it is completely cold.

The author delineates various configurations of colours produced by plates of different forms thus cooled. Among many others, a parallelogram of glass exhibits an inscribed parallelogram, with lines from each angle to the angles of the plate; and when the plate has been divided longitudinally by a diamond, each of the portions again exhibits an inscribed parallelogram, just as if the parts had been separately heated; and in this respect they present a property analogous to that of a divided magnet, each part of which has opposite poles as the entire bar.

A circular plate of glass, cooled with its centre resting on a piece of cold iron, or a cylindrical rod of glass cooled in the open air, when examined by polarized light in the direction of its axis, each present the same appearance of a black cross through their centre, and concentric fringes parallel to their circumference.

Since it is obvious that in these cases of rapid cooling, as well as those of rapidly heating, there must be progressive variations of density of the glass proceeding in a direction from the source of heat or of cold, and since the phenomena exhibited by many crystallized bodies, when examined in the direction of their axis, are precisely similar, Dr. Brewster infers that there exists in these crystallized bodies also a corresponding variation of density, proceeding toward their axes, which will afford an easy explanation of the fringes they exhibit.

Farther Experiments on the Combustion of explosive Mixtures confined by Wire-gauze, with some Observations on Flame. By Sir Humphry Davy, *LL.D. F.R.S. V.P.R.I.* Read January 25, 1816. [*Phil. Trans.* 1816, *p.* 115.]

In these experiments, the author examines what magnitude of wire and of apertures in the metallic gauze of his lamp is consistent with security against explosion of mixtures externally.

When the gauze is made of wire one fiftieth of an inch in diameter, and at intervals of one tenth, so as to make 100 apertures in the square inch, explosion may take place, either from intense ignition of the top of the lamp, or from lateral currents of air forcing the flame through the interstices.

When the intervals of the same wire were only one fourteenth, though the danger from lateral motion was obviated, still ignition of the wire caused explosion. With intervals of one sixteenth, still there was danger from the same source; but when the distances were reduced to one twenty-fourth on 576 apertures in the square

inch, then the lamp appeared safe, under all circumstances, in mixtures of coal gas and air.

With a view to explain the non-transmission of inflammation through small apertures, the author considers the nature of flame in general; and since a piece of phosphorus, or even a small taper, will burn in the midst of a large flame made by the combustion of alcohol, he is of opinion that oxygen exists in the centre of all flame, forming an explosive mixture with the vapour, but which burns solely at the exterior surface, because it is there alone sufficiently heated to take fire.

If a piece of wire-gauze be held in the flame of a lamp, or of coal gas, no flame passes through the gauze; for though a portion of the inflammable vapour passes, it loses too much heat in its passage to propagate the flame; but in the case of inflammable mixtures of coal gas entering a lamp, and burning at the interior surface, that which is exterior has not been exposed to any heating cause, and consequently is in no danger of taking fire at the sides of the lamp; and the results of combustion which escape at the top, though heated, are no longer inflammable.

In conclusion the author informs us, that these lamps have now been tried in two of the most dangerous mines near Newcastle with perfect success; and he has great hopes that they will shortly be adopted in many of the collieries in that neighbourhood.

Some Observations and Experiments made on the Torpedo of the Cape of Good Hope in the Year 1812. *By* John T. Todd, *late Surgeon of His Majesty's ship Lion. Communicated by Sir* Everard Home, *Bart. V.P.R.S.* Read February 15, 1816. [*Phil. Trans.* 1816, *p.* 120.]

The fish on which these experiments were made, were generally caught early in the morning, and examined as soon after as possible, but in some instances were kept in buckets of water as long as three days, or more.

They are frequently caught by the seine in Table Bay, to the westward of the Cape, but very rarely in Simon's Bay, which is to the eastward, and never caught by the hook with any kind of bait.

The Torpedo of the Cape differs in no respect from those of the Northern Hemisphere, except in size, which is never more than eight inches long and five in breadth. The columns of their electric organs appeared larger and less numerous than those described by Mr. Hunter. The form of any one singly is cylindric, but in a section of a whole organ the figure is modified by lateral compression.

The author found the supply of nerves to these organs, agreeably to former descriptions, to be larger than to any other parts.

The greatest shock they give was never felt above the shoulder. and rarely above the elbow joint, the strength of it depending more upon the vivacity of the animal than upon its size. There appeared

no regular interval between the shocks, which sometimes followed so quickly as not to be counted, while other animals could scarcely be provoked to give any shock.

The electric discharge was mostly accompanied by an evident muscular action in the animal, with an apparent swelling of the superior surface of the organs, and by a retraction of the eyes.

Two of these fish being placed in different buckets of water, one, which was irritated so as to give frequently repeated shocks, soon became languid, its shocks diminishing rapidly in intensity, and it soon died; but the other. not being irritated, continued living to the third day. And this was universally observable, that those which parted with shocks most freely soonest died.

A Torpedo, in which the nerves proceeding to the electric organs had been divided, seemed to have no power of giving shocks, but appeared just as lively as another Torpedo taken at the same time, and placed in a separate bucket of water uninjured.

Of two Torpedos taken at the same time, one had the electric organs divided. They were then both irritated equally, so that the perfect animal was soon exhausted of all power, and died; but the other, which had lost the power of giving shocks, appeared as vivacious as before, and lived to the second day.

An animal, from which one electric organ had been removed, was found still capable of giving shocks, though possibly not so strong as before.

Another fish, in which only one nerve to each organ had been divided, was also able to give shocks as before.

When they were held only by the tail or by the extremity of their lateral fins, they appeared to have no power of giving shocks.

Mr. Todd infers from these experiments,

That the electric discharge is a vital action.

That it is perfectly voluntary.

That frequent action is injurious to life, and may soon exhaust it.

That an animal deprived of this power is more vivacious, and lives longer than one which exerts this means of exhausting itself.

That both organs are not necessary for giving the shock.

That all the nerves of one organ are not necessary to be entire.

That a most intimate relation subsists between the nervous system and the electric organs.

Direct and expeditious Methods of calculating the Excentric from the Mean Anomaly of a Planet. By the Rev. Abram Robertson, *D.D. F.R.S. Savilian Professor of Astronomy in the University of Oxford, and Radcliffian Observer. Communicated by the Right Hon. Sir* Joseph Banks, *Bart. G.C.B. P.R.S.* Read February 15, 1816. [*Phil. Trans.* 1816, *p.* 127.]

Each of these methods, says the author, is to be considered as direct, although it proceeds through the medium of Cassini's approximation, which, as here used, can only be regarded as a first step

in the computation. No hypothesis is introduced into the process, and therefore no correction of error by trial is requisite.

Of three methods proposed, one combines the advantages of Keill's series and Cassini's approximation together, and is regarded by the author as the most simple in theory, and most expeditious in practice, which has yet been proposed.

Demonstrations of the late Dr. Maskelyne's *Formulæ for finding the Longitude and Latitude of a celestial Object from its Right Ascension and Declination; and for finding its Right Ascension and Declination from its Longitude and Latitude, the obliquity of the Ecliptic being given in both cases. By the Rev.* Abram Robertson, *D.D. F.R.S. Savilian Professor of Astronomy in the University of Oxford, and Radcliffian Observer. Communicated by the Right Hon. Sir* Joseph Banks, *Bart. G.C.B. P.R.S.* Read February 15, 1816. [*Phil. Trans.* 1816, *p.* 138.]

Dr. Robertson conceives that no full demonstration of these formulæ has yet been published; and hence no one has hitherto remarked two oversights with respect to their application to certain particular cases, which had escaped the notice of their author. Their value, however, has been duly appreciated by those most competent to judge of their merit, especially by M. Delambre, who remarks upon their conciseness, as well as precision, in comparison even with the formulæ given by Lalande.

Some Account of the Feet of those Animals whose progressive Motion can be carried on in opposition to Gravity. By Sir Everard Home, *Bart. V.P.R.S.* Read February 22, 1816. [*Phil. Trans.* 1816, *p.* 149.]

The power which flies have of crawling upon a ceiling is well known, but the mode in which this is effected, says the author, has never been explained. It was not till lately he learned that there are animals of a larger size which have the same power, and in which, from their size, the construction of their feet will admit of more accurate examination.

The *Lacerta Gecko* of Java walks up and down the smoothly polished chinam walls in quest of flies, and runs upwards to its retreat in the roofs of the houses, although the weight of a specimen given to the author by Sir Joseph Banks was as much as $5\frac{1}{4}$ ounces.

On the feet of this animal are five toes, armed with a very sharp and curved claw; and there are also on each sixteen transverse slits, with serrated edges, with pouches between them, which are considered by the author as the striking peculiarity in the foot of this lizard. When these are closed, the under surface of the foot bears a considerable resemblance to the upper part of the head of the sucking fish, the surface of which is furnished with two rows of moveable plates attached by one edge, and serrated at the other, and

its margin is surrounded by a broad loose membrane, capable of very close application to any surface on which it is placed.

By elevation of the plates, a degree of exhaustion is thus occasioned; and the fish thereby firmly attaches himself to the shark, or to any other object.

In the same manner it would appear that the transverse serratures of the bottom of the toes of the lizard, by their elevation, occasion a degree of exhaustion or partial vacuum, confined by the broad membrane which is attached all round each of the toes.

The author is of opinion, that the feet of the common fly act upon the same principle. Their under surfaces, when highly magnified, appear to be concave, as they are represented by Kellar; and he thinks it cannot be doubted that these cavities are employed to rarify the air between them and the surfaces to which they are applied, and thus support the weight of the fly, in opposition to gravity, when suspended from a ceiling.

On the Communication of the Structure of doubly-refracting Crystals to Glass, Muriate of Soda, Fluor Spar, and other Substances, by mechanical Compression and Dilatation. By David Brewster, *LL.D. F.R.S. Lond. and Edin. In a Letter addressed to the Right Hon. Sir* Joseph Banks, *Bart. G.C.B. P.R.S.* Read February 29, 1816. [*Phil. Trans.* 1816, *p.* 156.]

The subjects here chosen for experiment are such bodies as have in general no power of polarizing or depolarizing light, and the means employed for communicating these properties are purely mechanical. In the first instance, a piece of plate-glass was taken, and compressed edge-wise between two screws, and was found to polarize light in every part of its breadth, with depolarizing axes, making an angle of 45° with the edges of the plate.

When a narrow slip of plate-glass is attempted to be bent edgewise, the inner edge becomes compressed sufficiently to produce the effect of depolarization; and the exterior edge of the curve, by being dilated, also depolarizes: but the characters of the fringes of colour produced in the two cases are different; since those which arise from compression are such as are produced by calcareous spar and beryl; but those caused by dilatation of the exterior edge are such as appear from the action of sulphate of lime, quartz, and other bodies of that class.

The author observes, that the tints polarized ascend in Newton's scale, in proportion as the forces of compression or dilatation are increased.

When two plates under a state of compression are combined transversely, the same phenomena are exhibited as by means of a plate formed of a doubly-refracting crystal.

The effect of two plates of compressed glass, similarly placed, is the same as that of a double plate; but if they be placed transversely, then the tints are such as are due to the difference of their thick-

nesses: but the reverse will happen in each case if one glass be compressed and the other dilated.

If a compressing force be applied to the centre of a plate of glass, it will exhibit the black cross and other phenomena to be seen by means of doubly-refracting crystals.

If a plate so compressed be inclined to the polarized ray, the tints of colour will ascend or descend, according to the direction in which it is inclined.

If a plate to which the power of depolarization has been given by heat be compressed, the tints of the interior fringes rise in the scale, and those of the exterior descend, when the axis of pressure is perpendicular to their direction.

The same effects which are thus produced upon glass by compression, are produced in a similar manner upon such crystallized bodies as do not possess these properties in their natural state. But those bodies which already possess the doubly-refracting structure in a high degree, as calcareous spar, rock crystal, beryl, &c., suffer no change by any degree of mechanical compression to which the author has subjected them.

Since the tints of colour communicated to polarized light depends on the degree of force applied to glass, through which it is transmitted, Dr. Brewster conceives that a convenient instrument might be constructed for measuring the intensity of forces, which he would call a Chromatic Dynamometer; and in the same manner might variations of temperature, or humidity of bodies be measured, with the assistance of a little ingenuity, by chromatic thermometers and hygrometers.

In the prosecution of these experiments, the author examined the properties communicated to jelly by variations of its density from drying, and contrived means of giving it permanent power of depolarization, by the constrained position in which it was allowed to harden. And he found that the polarizing force of distended isinglass far exceeds any which can be given to glass, either by heat or pressure, and is even greater than that of beryl, which owes its power to crystalline texture.

In conclusion, the author expresses his hope that the principles here investigated afford a solution of the most important part of the problem of double refraction, by ascertaining the mechanical condition of both classes of doubly-refracting crystals, although the division of incident light into two portions oppositely polarized yet remains to be accounted for: and he thinks we must remain satisfied with referring this to the operation of some peculiar fluid, which he conceives to be the principal agent in producing all the phenomena of crystallization and double refraction.

Dr. Brewster adds, that a recent experiment (which he does not describe) upon the polarizing qualities of a body of which the densities vary in regular minute strata, induces him to think more favourably than heretofore of the undulatory system of light.

An Essay towards the Calculus of Functions. Part II. By C. Babbage, *Esq. Communicated by* William Hyde Wollaston, *M.D. Sec. R.S.* Read March 14, 1816. [*Phil. Trans.* 1816, *p.* 179.]

Experiments and Observations to prove that the beneficial Effects of many Medicines are produced through the Medium of the circulating Blood, more particularly that of the Colchicum autumnale *upon the Gout. By Sir* Everard Home, *Bart. V.P.R.S. Communicated by the Society for Improving Animal Chemistry.* Read March 21, 1816. [*Phil. Trans.* 1816, *p.* 257.]

The *Eau medicinale* of Husson, says the author, has most fortunately been discovered to be a specific remedy for the cure of gout; and he considers it to be now ascertained, by experiments of different persons, that this medicine is a vinous infusion of the *Colchicum autumnale*, or Meadow Saffron. He therefore hoped that an endeavour to ascertain its mode of action would be interesting to the Society for the Promotion of Animal Chemistry, whose objects are not confined to mere chemical combinations, but include the effects of galvanism on the nerves, and of mineral and vegetable solutions on the blood, and thereby on the healthy and morbid actions of life.

Although the late Mr. Hunter had ascertained, by experiments, that medicines injected directly into the circulating system produce in general the same effects as when taken into the stomach, he was not aware that even in the latter case (as has been proved by later experiments) these effects are not produced till they have reached the circulation by means of the absorbent system.

With respect to mercury, which appears to be the only specific hitherto known for any disease, it is completely established (says the author), by experiment, that this remedy, when in the circulation, is as effectual for the cure of recent chancre from the original application of venereal matter, as for constitutional symptoms in consequence of its absorption into the circulation.

It is observed, that the effect of *Colchicum autumnale* on gout is more rapid than that of mercury on the venereal disease, which admits of explanation, upon the supposition, that one is more rapidly received into the circulation than the other. With respect to the powers of this drug, the author speaks from experiments on his own person, having himself taken it at least six different times, for relief from local symptoms of gout, which it has removed, once in six hours, and at others in less than twenty-four hours.

With a view to ascertain the effects of this medicine introduced directly into the circulation, he substituted a dog as the subject of experiment. This dog's pulse in a natural state was 140 in a minute. In five minutes after injection of thirty drops of a vinous infusion of colchicum, diluted with a drachm of water, into the jugular vein, the pulse became fluttering, accompanied with a tremulous motion of the muscles, and nausea, but without retching to vomit. In less than a quarter of an hour the pulse intermitted, and was 180 in a

minute. In four hours the pulse had fallen to 120, but still intermitted. In seven hours the dog had a natural motion. Its pulse had returned to the natural standard of 140 in a minute, and he appeared perfectly recovered from the effects of the experiment.

After three days, sixty drops of the same infusion were given to the same dog to swallow. In two hours he became languid; his pulse wiry and weak, but still 140 in a minute. In four hours the languor was less, the pulse natural. In eight hours he had a natural motion, and in eleven hours appeared perfectly recovered from this dose.

The effects of this medicine upon the dog and upon Sir Everard Home, as far as they were sensible, were very similar, but differed in degree. Sir Everard, in consequence of an attack of the gout, took sixty drops of the *Eau medicinale*. In two hours he became hot and thirsty. In three hours the pain became tolerable while the limb was at rest. In seven hours he had a confined motion from the bowels. There was a degree of nausea. The pulse, which is naturally 80, was lowered to 60 in a minute, with occasional intermissions. In ten hours little remained except some degree of languor, with the pulse at 70, which on the following morning was restored to its natural standard, with removal of all symptoms of gout.

If these observations shall be confirmed, says the author, we must conclude that the different kinds of substances which produce specific diseases are first carried into the circulation, in the same manner as mineral and animal poisons; and that the medicines by which they are acted upon go through the same course before they produce their beneficial effects.

An Appendix to a Paper on the Effects of the Colchicum autumnale *on Gout. By Sir* Everard Home, *Bart. V.P.R.S.* Read April 25, 1816. [*Phil. Trans.* 1816, *p.* 262.]

In the paper on this subject, lately read to the Society, the author hoped to establish two facts which seemed to him of primary importance; first, that this medicine can be received into the circulation without permanent mischief; and secondly, that its beneficial effects upon gout are produced through that medium; and hence the sudden relief it gives can, he thinks, be readily explained.

The criterion by which he judges of the influence of the medicine is the pulse, which he has found to be invariably diminished ten or twenty beats in about twelve hours after it is taken; and since this effect is also produced when the same medicine is injected into a vein, he was satisfied that in the former instance the lowering of the pulse depends upon its arrival into the circulation, and not upon the state of the stomach.

The author has since been induced to try whether the effects of a larger quantity injected into the veins would also correspond with those produced by an overdose taken into the stomach.

One hundred and sixty drops of the infusion of colchicum before

employed, were now injected into the circulation of a dog. He instantly lost all power of motion; the breathing became slow, the pulse hardly to be felt. In ten minutes it was 84; in twenty minutes at 60; in an hour at 115, with the respiration so quick as scarcely to be counted. In two hours the pulse was 150, and very weak. In the mean time the animal was purged; and he vomited, first a bilious fluid, and then bloody mucus; and after lingering in an extremely languid state five hours, expired.

On dissection, the internal coat of the stomach and intestines were found inflamed in a greater or lesser degree universally.

The facts here adduced, says the author, go as far as it is possible to prove that the action of *Colchicum autumnale* on the different parts of the body is through the medium of the circulation, and not in consequence of its immediate effects on the stomach.

On the Cutting Diamond. By William Hyde Wollaston, *M.D. Sec. R.S.* Read May 2, 1816. [*Phil. Trans.* 1816, *p.* 265.]

The author, having never met with any satisfactory explanation of the property which the diamond possesses of cutting glass, has endeavoured, by experiment, to determine the conditions necessary for this effect, and the mode in which it is produced. The diamonds chosen for this purpose are naturally crystallized, with curved surfaces, so that the edges are also curvilinear. In order to cut glass, a diamond of this form requires to be so placed that the surface of the glass is a tangent to a curvilinear edge, and equally inclined laterally to the two adjacent surfaces of the diamond. Under these circumstances the parts of the glass to which the diamond is applied are forced asunder, as by an obtuse wedge, to a most minute distance, without being removed; so that a superficial and continuous crack is made from one end of the intended cut to the other. After this, any small force applied to one extremity is sufficient to extend this crack through the whole substance, and successively across the whole breadth of the glass. For since the strain at each instant in the progress of the crack is confined nearly to a mathematical point at the bottom of the fissure, the effort necessary for carrying it through is proportionally small.

The author found by trial that the cut caused by the mere passage of the diamond need not penetrate so much as $\frac{1}{200}$th of an inch.

He found also that other mineral bodies recently ground into the same form are also capable of cutting glass, although they cannot long retain the power, from want of the requisite hardness.

An Account of the Discovery of a mass of native Iron in Brasil. By A. F. Mornay, *Esq. In a Letter to* William Hyde Wollaston, *M.D. Sec. R.S.* Read May 16, 1816. [*Phil. Trans.* 1816, *p.* 270.]

This mass was first discovered in the year 1784 by a person of the name of Bernardino da Mota Botelho, while looking after his cattle;

and in consequence of the account he gave, the Governor-General of the province ordered it to be brought down to Bahia. A stout waggon or truck was conveyed to the spot; and after three days' labour, the mass was lifted upon it, and by the force of forty pair of oxen was removed about a hundred yards, as far as the bed of a neighbouring rivulet, but there relinquished. And there it was found again by Mr. Mornay in January 1811; but the river was at that time quite dry, and frequently is so.

The mass is about seven feet in length, its breadth four, with a thickness of about two feet. Its form is very irregular, with various cavities, especially on the under side; but the author estimated its capacity on the spot to be full twenty-eight cubic feet; and hence the weight is conceived to be about 14,000 pounds.

Its colour is that of a dark chesnut, rather glossy on the top and sides; on its underside it is covered with a crust of oxide in thick flakes.

In some of the smaller cavities were imbedded quartz pebbles, too large to be taken out without being broken. The block *in situ* appeared to be highly magnetic, having its north pole lying in a N.E. direction; but the fragments separated from it were not found by Mr. Mornay to possess magnetic poles. In detaching these fragments he experienced very considerable difficulty, although aided in some measure by a crystalline texture, which gave direction to the fractures, and was visible in all the specimens he could obtain with a sledge-hammer carried for that purpose.

Having dissolved a small portion of this iron, and examined the solution by such re-agents as he happened to possess, he thought that he discovered the presence of nickel, but very doubtfully, and in very small quantity. He also made a similar examination of some fragments from a bed of oxide found where the mass had been originally discovered, and with a similar result as to an apparent trace of nickel.

The surface of the country was at this spot covered with a coarse gravel to the depth of ten or fifteen feet above the level of the rock of granite, which in general prevails.

The latitude of the place was estimated by Mr. Mornay to be 10° 20′ S., and the longitude about 33′ 15″ W. of Bahia.

To the southward were found prismatic fragments, and balls from a few inches to nearly three feet in diameter, supposed to be basalt; and beyond these, at the distance of forty leagues, a range of sandstone hills, from which there extends a sandy plain with occasional elevations, all about twenty fathoms in height, as if they were the remains of some more elevated plain, of which certain parts were more durable from a cement of iron that appears in the beds of most of them. A peculiar aspect is also given to this plain by other smaller hillocks, which are very numerous, and are the nests of the white ants; these are conical in their outline, but almost invariably elliptic at their base.

The soil of the valleys is observed to be impregnated with sea-

salt, which the inhabitants wash out for their own consumption; but it is very impure, from a mixture of bitter purgative salt along with it.

There are here many warm springs, of which several are as much as 8° above the surrounding atmosphere; and one of them was at 90°, while the temperature of the air was only 73°.

Among the vegetable productions of this country observed by Mr. Mornay, is one very remarkable for the light which it yields when cut. It is described as a climbing plant, destitute of leaves, and without thorns. It contains a milky juice, which exudes as soon as the plant is wounded, and appears luminous for several seconds. This juice is extremely acrid, so as to blister the skin, even of quadrupeds, to which it is applied. It becomes viscid by drying, and turns of a greenish-yellow colour, appearing to be a gum-resin.

It is conjectured that this plant may be an Euphorbium; but as the author had not the good fortune to find it in flower, he had not the means of learning with certainty to which tribe of plants it may belong.

Observations and Experiments on the Mass of native Iron found in Brasil. By William Hyde Wollaston, *M.D. Sec. R.S.* Read May 16, 1816. [*Phil. Trans.* 1816, *p.* 281.]

The observations here added as an Appendix to Mr. Mornay's paper, relate to the crystalline texture of the mass, and its magnetic qualities. The experiments were undertaken for the purpose of ascertaining the presence of nickel more certainly than Mr. Mornay had been enabled to do, aud also to discover the quantity of this ingredient.

The crystalline forms in which this iron breaks, are such as are common to many metallic substances; the regular octohedron, tetrahedron, or rhomboid, consisting of these two combined.

With respect to magnetism, it is observed, that the fragments are precisely like any pieces of the best soft iron, easily obeying the magnet, and instantly assuming polarity, according to the direction in which they are placed with reference to the magnetic meridian, and as instantly changing it when reversed. And hence it is inferred that the magnetic polarity of the mass is solely from similar induction.

For detecting the presence of nickel, the author dissolves a very small portion in nitric acid, evaporates to dryness; and after dissolving the oxide of nickel in pure ammonia, he precipitates by triple prussiate of potash.

But for determining its quantity, he adds sulphuric acid to the ammoniacal solution; and after evaporating to dryness, he expels the ammoniacal salts by heat, and then dissolves the residuum, which is mere sulphate of nickel, and, by crystallization, obtains it in a state from which the quantity of metallic nickel can be inferred. By this process he found four per cent. of nickel in this specimen of native

iron, and a corresponding quantity in the scales of oxide detached from its cavities.

On Ice found in the bottoms of Rivers. By Thomas Andrew Knight, *Esq. F.R.S. In a Letter addressed to the Right Hon. Sir* Joseph Banks, *Bart. G.C.B. P.R.S.* Read May 23, 1816. [*Phil. Trans.* 1816, *p.* 286.]

The author having witnessed this phenomenon in the course of last winter in the river Teme, which runs past his residence in Herefordshire, describes the appearances that he observed, and relates the circumstances under which they occurred, for the purpose of accounting for a fact which, though frequently noticed, has not yet been satisfactorily explained.

After a night that had been intensely cold, the stones in the rocky bed of the river glistened with a kind of silvery whiteness, which, upon examination, arose from numerous spicula of ice adhering to them, and crossing each other in every direction. The river was not at that time frozen over in any part, but the temperature of the water was at the freezing point; and in a mill-pond just above, the water was replete with millions of spicula of ice, which naturally would have a tendency to rise and form a crust at the surface; but in falling over a low weir into a narrow channel, numerous eddies, occasioned by large projecting stones, constantly carried fresh spicula to the bottom, where they collected against the surfaces and in the cavities opposed to the current, especially in those parts where it became less rapid.

Had the coldness of the weather continued, it is conceived that the ice might have continued to accumulate to much larger quantities, as it had been known to do in the same situation some years preceding, when the frost was of long duration.

It was remarked by Mr. Knight, that near the shore the ice that adhered to stones partly out of the water had a firmer consistence, although apparently originating from the same source. This ice extended as far as half a yard from the shore, and was three or four inches below the surface of the water. This did not melt so rapidly as that which was deposited at greater distances from the sides, and at greater depths.

Although the existence of porous ice in any large quantities may thus be explained in larger rivers, where there are eddies sufficient to carry floating spicula in contact with the bottom, yet the author expresses his doubts respecting large masses of solid ice said to have been found at the bottoms of deep and sluggish rivers, in which there are no eddies to cause the descent even of small particles, and no obvious cause of subsequent consolidation.

On the Action of detached Leaves of Plants. By Thomas Andrew Knight, *Esq. F.R.S. In a Letter addressed to the Right Hon. Sir* Joseph Banks, *Bart. G.C.B. P.R.S.* Read June 13, 1816. [*Phil. Trans.* 1816, *p.* 289.]

Mr. Knight having on a former occasion inferred, from his experiments and observations, that the true sap of trees, from which the living parts are generated, owes its properties to having circulated through the leaves, now adduces other facts, more directly in point, to show that a fluid similar to the true sap actually descends through the leaf-stalks.

A transverse section was made through the bark of a vine, at the middle of the insertion of the leaf-stalk, by passing a slender knife through the stalk, so as to split it for about two thirds of an inch above its insertion.

Another transverse incision of the bark having also been made nearly an inch below, these sections were joined by longitudinal incisions at each end, so that a piece of the bark, half an inch broad and nearly one inch long, could be detached from the stem, still remaining united to the lower half of the split leaf-stalk. Being afterwards protected on all sides from the air by waxed paper, it was found to grow in all its dimensions, and to have thin layers of alburnum deposited upon its interior surface.

In a second experiment, leaves from the potatoe were taken at the period when the tuberous roots were beginning to form, and were planted in pots, under the expectation that these leaves even alone might have power to form tubers. The effect, however, was not exactly as the author had anticipated; but the power was manifested by the production of a conical swelling at the lower part, more than two inches in circumference, apparently similar in composition to a tuber, and retaining life to the following spring.

Leaves of mint, also detached in the same manner, were found to throw out roots, and to continue alive through the winter, assuming the character and hue of those of evergreen trees.

Since it had appeared, from former experiments, that the growth of immature leaves depends upon matter afforded by those already arrived at a state of maturity, Mr. Knight cut off several shoots of a vine, and laying them over basins of water, immersed portions of the larger leaves; and he found that under these circumstances the young leaves continued to grow for upwards of a month, during which they necessarily depended on the larger leaves for their supply of nourishment.

The progress of fruit, likewise, is proved to depend upon the mature leaves; for if these be destroyed, the fruit ceases to grow, and gains nothing in ripeness or flavour: and, accordingly, those trees alone are capable of ripening fruit during winter which retain their leaves at that season, of which the Orange, Lemon, Ivy, and Holly, are familiar examples.

With regard to the period during which the true sap is accumu-

lated as store for future growth, and returned from its reservoirs into the circulation, it may be difficult clearly to discover anything certain; but the author has not ceased to prosecute his experiments on the varying density of the alburnum, and other parts of the wood, and on the proportion of moisture which they lose by drying; and he hopes at some future time to lay before the Society his observations, showing how far the durability of the heart wood depends on the period at which a tree is felled.

On the Manufacture of the Sulphate of Magnesia at Monte della Guardia, near Genoa. By H. Holland, *M.D. F.R.S.* Read June 13, 1816. [*Phil. Trans.* 1816, *p.* 294.]

The site of this manufactory is about eight miles N.W. of Genoa, at about 1600 feet above the level of the sea, from which the top of the mountain is five miles distant, and elevated about 2000 feet. The ascent from Sestri is by a deep ravine, the course of a torrent, the eastern side of which is composed of serpentine in vast masses, lying unconformably on primitive schist, and containing talc, steatite, asbestus, and many small veins of pyrites. On the western side of the ravine are mountains of magnesian limestone. In passing to the upper end of this ravine, the stratification of the primitive schist appears mixed with chlorite, slate, and other magnesian minerals, and containing numerous veins or layers of pyrites, both of copper and iron. The substance of these ores is schistose, as well as the rock in which they lie, and they are so intimately mixed with the same magnesian minerals, as to feel unctuous to the touch. These, together with a certain portion of magnesian limestone, are the materials used in the manufacture of the sulphate of magnesia, in an establishment originally set up for converting copper and iron pyrites into sulphates of those metals.

The sulphate of magnesia was at first observed only as an accidental product, but has now become the principal object of the work. For this purpose the pyrites is extracted from the mountain by tunnels, the largest of which is about 200 feet in length, and from 10 to 15 feet wide. The ore is then broken into small pieces, roasted for about ten days, and being then collected in heaps, is kept moist with water for several months, during which the salts are forming. The materials are then lixiviated, and after the liquor has been filtered through sand, the copper is first precipitated by refuse iron, after which a portion of lime, prepared from the magnesian limestone of the adjacent mountain, is added, in order to precipitate the iron, and at the same time to make some addition to the product of sulphate of magnesia.

The circumstance particularly to be attended to in this process, is the proportion of lime employed, which in general does not exceed $\frac{1}{40}$th of the weight of ore. For if this were added in excess, it would occasion the precipitation of the magnesia along with the metals. The whole produce of this manufactory, we are told, does

not exceed 1½ cwt. per week; but it is of very good quality, and is used extensively in Italy under the name of Sal Inglese.

On the Formation of Fat in the Intestine of the Tadpole, and on the Use of the Yolk in the Formation of the Embryo in the Egg. By Sir Everard Home, *Bart. V.P.R.S.* Read May 23, 1816. [*Phil. Trans*. 1816, *p*. 301.]

From the smallness of tadpoles in this country, they have not attracted the notice of naturalists so much as their peculiarities deserve. But those of the *Rana paradoxa* of Surinam being of a much larger size, are fitter subjects for observation. The tadpole of this frog bears so strong a resemblance to a fish, that it is commonly sold as such for the use of the table. But as these are not to be had here in sufficient quantity for examination, the author had recourse to the common tadpole of this country.

This animal, as soon as it leaves the ovum, has ten filaments projecting from the neck on each side, which answer the purpose of gills. In the young shark, while yet in the egg, there are twenty-four similar filaments to answer the same purpose. In the common newt also is a similar apparatus, but the number is only three on each side. In each instance this structure is but temporary, and drops off when the permanent structure of lungs in the frog, and of gills in the shark, is completely formed.

During the growth of the tadpole its abdomen becomes distended, the intestine being then very capacious, and filled throughout its whole extent with a soft substance, that burns with a vegetable smell. Behind the intestine, along the posterior part of the abdomen, is accumulated a quantity of fat of a yellow colour, inclosed in long thin transparent membranous bags. During the conversion of the tadpole into the frog by development of the legs, lungs, and other organs before wanting, the whole of this fatty matter becomes absorbed, in the same manner as the yolk of the hen's egg is taken up during the progressive growth of the young chicken. So that although the egg of the frog differs from that of other animals of the same class in having no yolk, a substance corresponding to it appears to be necessary previous to the formation of bones, and other more solid parts of the perfect frog. For the production of this matter, it is observed that the tadpole is provided with a most uncommon length of intestine, which contracts to one of ordinary size as soon as the full supply of fat is generated.

The author adds the result of various chemical experiments, made by Mr. Hatchett and Mr. Brande on the spawn of the frog, from which it appears, that it is of a nature between gelatin and albumen; that it contains no concrete oil like that of the hen's egg; that the ova of the lizard and snake, and of cartilaginous fishes, have, on the contrary, yolks which do contain a concrete oil, that in its nutritive qualities corresponds to the butyraceous part of milk with which the young of viviparous animals are supported for a certain time after birth.

From these observations, and from the peculiar provision of fat laid up by the tadpole previous to its metamorphosis into the frog, Sir Everard Home is led to consider a certain portion of oil as necessary for the formation of bone, and to observe that the proportion of fat in different ova, corresponds with the greater or less degree of hardness of bone that is about to be produced.

On the Structure of the Crystalline Lens in Fishes and Quadrupeds, as ascertained by its Action on Polarized Light. By David Brewster, *LL.D. F.R.S. Lond. and Edin. In a Letter addressed to the Right Hon. Sir* Joseph Banks, *Bart. G.C.B. P.R.S.* Read June 20, 1816. [*Phil. Trans.* 1816, *p.* 311.]

The author having found that in many instances depolarization depends upon variations of density in the bodies through which it is transmitted, concluded that corresponding effects would be produced by the crystalline lens of the eye, which is well known to increase in density towards the centre.

By immersing the crystalline lens of a cod in Canada balsam, the refraction at its exterior surface was so far removed, that the effects of its internal texture could be examined independent of its external spherical form. Under these circumstances, when it was exposed to polarized light, with its axis of vision parallel to the beam of light, it exhibited three concentric circles of light separated by two dark circles, and intersected by a black cross so as to be divided into twelve luminous sectors. By varying the inclination of the axis, these appearances varied in a manner that can scarcely be described without the assistance of the drawings which accompany this paper.

By removing successively the capsule and outer portions of the lens, the exterior circle of light was first obliterated; and then the second disappeared, so that ultimately there remained only the central light intersected by a black cross.

On examination of the variation of tints of colours produced by the combined effect of the crystalline with a plate of sulphate of lime, it appeared to the author that the central nucleus, and an exterior spherical coat, are in a state of dilation, while the intermediate coats are in a state of contraction.

In the crystallines of sheep and oxen, the author observed a correspondent texture; but in these there appeared only one series of luminous sectors.

By examination of the cornea also, Dr. Brewster found a texture similar to that of the nucleus of the crystalline, both in fish and quadrupeds; but the sclerotic coat has merely a confused power of depolarization, similar to that of a mass of crushed jelly of isinglass, a property which does not really belong to its whole substance, but solely to a thin membrane that covers it externally.

From these experiments Dr. Brewster infers, that all the parts of the crystalline of fishes correspondent to the two dark concentric circles, exercise no action upon polarized light; that a central nu-

cleus and an external spherical shell are in a state of dilatation, and that a shell intermediate between them is in a state of contraction; that its structure and optical properties are not alike in all directions, but have reference to the axis of vision; and that its peculiar structure probably is necessary for correcting spherical aberration.

Some farther Account of the Fossil Remains of an Animal, of which a Description was given to the Society in 1814. *By Sir* Everard Home, *Bart. V.P.R.S.* Read June 13, 1816. [*Phil. Trans.* 1816, *p.* 318.]

The present descriptions are taken from specimens in the collection of Mr. Buckland of Oxford, and Mr. Johnson of Bristol, and are thought to determine the class to which this animal belongs.

The structure of the vertebræ, as shown in the author's former paper, made it evident that the progressive motion of the animal resembled that of fishes; but at that time neither the scapula in its perfect state had been seen, nor had the bones of the pectoral fin been found, which now make it clear that all the bones correspond with those of fishes, but differ essentially from those of land animals.

In all animals that breathe by means of lungs, the ribs are articulated both to the bodies and to the transverse processes of the vertebræ, so as to admit of expansion of the chest; but the ribs of fishes which solely give defence to the viscera, have only one insertion, being connected solely with the bodies of the vertebræ, midway between their two articulating surfaces, so as not to interfere with the motion of the vertebræ on each other, as is the case in land animals.

The author observes, that the ribs in this animal are placed in all respects like those of fish.

In the whale tribe the scapula and bones of the fore fin resemble those of the crocodile, and they bear a close analogy to those of land animals in general; but in this animal these bones, it is observed, resemble those of the shark.

It is remarked also, that the bones in the young state have epiphyses, as is the case with the bones of fish generally. The ribs also appear to have been soft like those of fish, as we may judge from the grooved or fluted form, they have assumed by compression.

But though, from consideration of all these circumstances, Sir Everard Home has no doubt that this animal was a fish, he admits that the long projecting snout and conical teeth show a marked difference between this animal and any now in existence, and may occasion a difficulty in arranging it with any class of known animals.

Farther Observations on the Feet of Animals whose progressive Motion can be carried on against Gravity. By Sir Everard Home, *Bart. V.P.R.S.* Read June 27, 1816. [*Phil. Trans.* 1816, *p.* 322.]

Since the author's former observations on this subject were communicated to the Society, he has been enabled, by the assistance of

Mr. Bauer, to correct his former representations of the structure of the feet of the *Lacerta Gecko* and House Fly, and also to add an account of a similar mechanism in other insects delineated by the same skilful artist.

In the Blue-bottle Fly the suckers are two on each foot, immediately under the root of each claw. Their form is funnel-shaped, with a narrow neck; when not in use they are contracted, and approaching each other lie together in the space between the claws; but when prepared for action they are expanded, and they separate laterally from each other.

In the Horse Fly (the Tabanus of Fabricius,) the suckers differ from the former only in number, as there are three on each foot.

In the yellow Saw Fly (the *Cimbex lutea* of Fabricius,) suckers are found at each of the four joints of the toes, one at every joint.

In the *Dytiscus marginalis*, or Great Water Beetle, although for the common purpose of locomotion there is no such apparatus observable at the parts of the feet used in walking, the male is nevertheless abundantly furnished with suckers in the three first joints of the first and second pair of feet; and the purpose to which they are applied is that of embracing the female, who does not want, and is not provided with a similar mechanism. In the male those joints that are furnished with suckers, are of uncommon breadth, having the whole under surface covered with small suckers, to each of which is a small tubular neck.

In addition to the foregoing observations on the suckers by which insects attach themselves to different objects, the author also takes occasion to notice another peculiarity in the feet of some insects, as security against the injury they might sustain in alighting suddenly with considerable velocity.

In some species of Grylli and Locustæ the feet have on their under side globular cushions filled with a fibrous substance, and possessed of considerable elasticity.

In the *Locusta varia* there are three pair of cushions, of different sizes, at the three first joints of each leg.

But in fleas, which the author examined under an expectation of finding a similar structure, nothing of this sort is to be found; from which it would appear that such a provision is not wanted for resisting the momentum of so light a body, notwithstanding the great distance to which it leaps.

A new Demonstration of the Binomial Theorem. By Thomas Knight, *Esq. Communicated by* W. H. Wollaston, *M.D. Sec. R.S.* Read July 4, 1816. [*Phil. Trans.* 1816, *p.* 331.]

On the Fluents of irrational Functions. By Edward Ffrench Bromhead, *Esq. M.A. Communicated by* J. F. W. Herschel, *Esq. F.R.S.* Read June 4, 1816. [*Phil. Trans.* 1816, *p.* 335.]

An Account of the Circulation of the Blood in the Class Vermes of Linnæus, and the principle explained in which it differs from that in the higher Classes. By Sir Everard Home, *Bart. V.P.R.S.* Read November 7, 1816. [*Phil. Trans.* 1817, *p.* 1.]

The circulation of the blood in these animals is different from that of any other animal known to the author. There is one point to which all the blood is brought, and from which it is again emitted to all parts of the body. To this the author gives the name of heart, although it be not, in fact, the principal agent in carrying on the circulation, and although so small as hardly to deserve the name of ventricle. It is situated directly in the middle line of the belly, and is that point in a vessel which comes from the head, where the rest of the blood is received from two auricles that lie above it at the back of the animal, one on each side.

Sir Everard, after describing the unsuccessful attempts which he had made to discover the arrangement of the vessels by transparency of the body of the animal in full sunshine, acknowledges that he is indebted to Mr. Clift for the means of detecting the course of its circulation, by steeping it in vinegar, which presently coagulates the blood in the vessels, and gives it a deep black colour.

From the central point before mentioned, the artery proceeds down towards the tail, giving off branches by pairs to the bronchiæ as it passes, which may be termed pulmonary arteries. From these the blood is returned to a single vein, which passes up the middle of the back to the head. From the external situation of the bronchiæ, the circulation of the blood in them is readily seen, and is described not to be simultaneous in adjacent pairs of them, but successive from the head backwards. The blood propelled forward by these toward the head, is returned from thence by the longitudinal vein along the belly to the original centre of motion.

In the mean time, another portion of the blood which has supplied the viscera and skin, is returned by two large veins that pass up on each side of the animal, and is by them delivered to the two auricles mentioned as placed in the middle of the back.

The great peculiarity in the circulation of this animal, appears to consist in the muscular structure of the branchial vessels. Of these there are as many as thirteen pair, which appear abundantly sufficient, in proportion to the size of the animal, for the aeration of the blood, as well as for propelling it forwards.

In the *Lumbricus terrestris* the circulation of the blood, and the mode in which it is aerated, the author says, are very different. A large artery passes along the belly, giving off lateral branches, and a corresponding vein runs up the back, with its branches on each side. But the two great trunks also communicate laterally through five pair of reservoirs, filled from the vein, and emptying themselves into the artery. On each side of the great vein on the back, are also a row of vesicles with external openings, for the purpose of aerating the blood, as shown in the drawing intended to accompany this paper.

The author observes generally, respecting the Vermes of Linnæus, that the blood is conveyed by veins to the respiratory organs, and from thence by arteries to the heart, in which they differ from fishes; while they differ, on the other hand, from Mollusca in general, by having red blood.

The Sepia has been thought to have a circulation peculiar to itself; but the author remarks, that it bears a close resemblance to that of the Teredo, in having two auricles which transmit the blood received from the bronchiæ to the heart, but differs in this, that there are also two auricles that receive the venous blood, and transmit it to the respiratory organs.

Observations on the Hirudo vulgaris. *By* James Rawlins Johnson, *M.D. F.L.S. &c. Communicated by the Right Hon. Sir* Joseph Banks, *Bart. G.C.B. P.R.S.* ead Nov ember 14, 1816. [*Phil. Trans.* 1817, *p.* 13.]

The animal here described under the name of *Hirudo vulgaris*, is the same that has been denominated by Linnæus and others, *Hirudo octoculata*, on account of the number of its eyes; but since the *Hirudo tessulata* has also eight eyes, it has been thought proper to change that name for one not liable to mislead.

This little animal is found very commonly in rivulets, attached to the under surface of stones. Its length varies from one inch to an inch and an half. The back is of a dark brown, marked with numerous transverse lines, and a central line longitudinally of a black colour. On the belly also is a longitudinal black line, but the rest is of a yellowish-green colour.

In structure this animal very much resembles the *Hirudo medicinalis*, having the anus at the extremity of the tail, and four longitudinal vessels destined to convey the circulating blood, one dorsal, one abdominal, and two lateral. In these, eight pulsations are observable in the course of a minute, but not derived from any central organ corresponding to the heart of other animals.

Its food consists of very small worms, which it swallows whole.

The object of the present communication is to record such peculiarities as the author has observed in its mode of propagation, to which he has paid attention during the last two summers.

Having found a pair of them copulating as hermaphrodites, like the common snail, on the 13th of August, he watched the period at which eggs were deposited, and found a capsule of eggs produced on the 17th, and another on the 18th, both of which were subsequently hatched. The same leech also laid as many as seven more capsules, at intervals of two or three days in succession, but all these last seven proved abortive.

Those which were productive showed signs of life in three weeks, and in five more the young made their escape from the capsule.

The capsule in which the eggs are deposited is formed as a membranous ring, surrounding the body of the parent in the region of

the uterus, at the same time that the eggs are forming within the uterus. When the animal is about to produce a capsule, it fixes itself by the tail, and in the course of ten minutes is seen to become much distended in the region of the uterus, but contracted both above and below that part. The swelling at first has the ordinary dark colour of the animal, but in a few minutes a film is seen to separate, and become of a milky white colour, from the contents of the uterus, which are forcibly emitted into it. The animal itself, being thereby diminished, next loosens itself from the enveloping membrane by forcible elongation of the fore part of the body, and then withdraws its head backwards, as from a collar, leaving two openings in the capsule, which, after contraction, remain visible as dark specks, one at each end. These are the points at which the young ultimately make their escape, being apparently aided by the comparative weakness of these parts of the membrane. At the time that they are hatched the young are nearly colourless, and they continue so for several months with very little enlargement. While young they have the property of swimming at the surface of the water with their bellies uppermost, as has been noticed by Müller in the *Hirudo hippoglossi*, and as the author has also noticed in two other species of Hirudo.

On the Effects of Galvanism in restoring the due Action of the Lungs. By A. P. Wilson Philip, *Physician in Worcester. Communicated by Sir* Everard Home, *Bart. V.P.R.S.* Read November 21, 1816. [*Phil. Trans.* 1817, *p.* 22.]

The author ascribes our having derived but little advantage hitherto from the employment of galvanism in the cure of disease, to want of discrimination with regard to the functions of the nervous system, which he considers as twofold, one properly nervous, the other purely sensorial.

Galvanism, he says, never did perform any of the functions of the *sensorial* system; it cannot restore hearing to the deaf, or sight to the blind; and yet these are the cases that have been blindly selected for its employment. On the muscles it acts purely as a stimulus, and is not to be expected to do more than other stimuli. But since it appears to have peculiar power over the nervous system, he was led to inquire what diseases depend on a failure of nervous influence; and from having observed the difficulty of breathing brought on by dividing the eighth pair of nerves, and the relief afforded in that case by sending a stream of galvanism through the lungs, he was induced to try the effects of galvanism in habitual asthma, or asthmatic dyspnœa, which he conceived to depend on some obstruction of nervous energy.

In such instances as have come under his own observation, the employment of galvanism has been almost uniformly attended with relief to the symptoms, and in many instances has proved a perfect cure. When it is applied as strong as the patient can well bear without complaint, the relief is often perceived in five minutes, and

generally in less than a quarter of an hour. The battery employed consisted of 4-inch plates, charged with a mixture of muriatic acid and water, in the proportion of one acid to twenty of water.

With regard to the number of plates, there are few persons who cannot bear so many as eight pair, and few who require more than twice that number. Dr. Philip generally begins with a very weak power, and gradually increases it till the patient begins to feel some degree of uneasiness, when it is easy to move the wire of communication backward a few divisions, and again return after a temporary remission of the excitement. The application was made through the medium of two thin plates of metal dipped in water and applied wet, one to the nape of the neck, and the other to the pit of the stomach, in order that the galvanic fluid might pass through the chest as nearly as possible in the direction of the nerves; and with respect to the position of the two ends of the battery, it was found expedient to place the positive wire to the nape of the neck, and the negative wire to the pit of the stomach. The operation was discontinued as soon as the patient felt his breathing become free; for it was not found that continuance for a greater length of time had any tendency to prevent a recurrence of the symptoms.

In those cases of spasmodic asthma in which galvanism has been applied by the author, it has failed to give relief; but where the spasmodic paroxysm has been succeeded by habitual difficulty of breathing, there galvanism did give immediate, but temporary relief. It appears, however, that most is to be expected from this remedy in those cases of habitual asthma which are the least complicated with any other disease.

In order to be assured that the effects above described, as apparently arising from galvanism, might not, in fact, be owing to an impression on the mind during its employment, Dr. Philip endeavoured in various instances to deceive his patients, by imitating the pricking sensation occasioned by the application of the wires to the surface of the skin without really completing the circuit; but they invariably received no relief from such experiments, and very soon felt the customary benefit after the communication with the trough had been established in the usual way.

In one instance the galvanic current was directed solely along the spine, by placing the second wire to the small of the back instead of the pit of the stomach; but the advantage derived from this application was far less than in the usual mode.

In confirmation of the truth of these observations, the author refers to various professional friends who have witnessed the trials, and especially the House Surgeon of the Worcester Infirmary, who is convinced that no other means are equally efficacious in the relief of this species of asthma

Account of some Experiments on the Torpedo electricus, *at La Rochelle. By* John T. Todd, *Esq. Communicated by Sir* Everard Home, *Bart. V.P.R.S.* Read December 5, 1816. [*Phil. Trans.* 1817, *p.* 32.]

Having upon a former occasion submitted to the Royal Society some experiments and observations on the *Torpedo electricus*, the author is now induced to offer a continuation of his researches; and in the present communication describes a series of experiments undertaken with a view of ascertaining whether that animal possesses any power either of exciting the electrical organs, or of interrupting their action independent of the large system of nerves, by which they are directly supplied. The commencement only of this investigation is now submitted to the Society, the author having been deterred from its completion by untoward circumstances.

His experiments were performed immediately after the fish was taken, and while it was vivacious and active. When the lateral cartilages and all their appertaining muscles were divided, the shocks seemed as potent as before such operation. Neither were the powers of the electrical organ sensibly diminished by removing its superior surface, nor by making a deep vertical incision into it. Even when one half of each electrical organ was removed, the power of giving shocks was retained by the remainder.

These experiments were performed on two torpedos; the one eight, and the other eighteen inches in length. The results were in all main points similar; but the smaller fish became, as might have been expected, most speedily exhausted.

In a third torpedo, between nine and ten inches long, an incision was made round the circumference of both organs, so as to leave no attachment between them and the animal, except by the nerves; but the power of giving shocks was not impaired by this operation. The author remarks that the nerves supplying the electrical organs of the torpedo arise exclusively from the medulla oblongata, notwithstanding the long course which some of them take before they reach the organs.

The torpedo called by the lower orders in France *la Tremble*, is abundantly taken between the mouths of the Seine and the Garonne, and forms an article of food among the poorer inhabitants; who, however, carefully avoid the electric organs, which they consider as noxious.

A Description of a Process, by which Corn tainted with Must may be completely purified. By Charles Hatchett, *Esq. F.R.S. In a Letter addressed to the Right Honourable Sir* Joseph Banks, *Bart. G.C.B. P.R.S. &c. &c.* Read December 5, 1816. [*Phil. Trans.* 1817, *p.* 36.]

The great loss formerly experienced by the mustiness of imported grain, led the author, some years ago, to the means now described of removing the taint, and which he conceives may be advantageously

applied to the large quantities of corn which were unavoidably housed in a damp state, in consequence of the unpropitious weather, during the late harvest. The author considers the mustiness to be confined principally to the exterior amylaceous part of the grain, and the process proposed consists in pouring upon the tainted grain thrice its quantity of boiling water. When cold, the water and floating grains are to be poured off; the corn is to be washed with cold water, drained, and carefully kiln-dried. It will be found perfectly sweet, and the loss of weight is inconsiderable.

The advantages of this process are its simplicity and cheapness; and although the author has hitherto only applied it to wheat, there can, he observes, be little doubt that oats and other grain may be deprived of must with equal success.

Observations on an astringent Vegetable Substance from China. By William Thomas Brande, *Esq. Sec. R.S.* Read December 12, 1816. [*Phil. Trans.* 1817, *p.* 39.]

The substance described in this communication was sent to Sir Joseph Banks as a species of galls, used by the Chinese in dyeing black. They have the appearance of irregular vesicles, of a purely astringent flavour, and closely agree with those described by Du Halde under the name of *ou poey tse*, which are also employed in China as the bases of many astringent medicines.

By digestion in cold distilled water, these galls yielded a pale brown infusion, of a highly astringent taste, and furnishing a copious white precipitate with solution of animal jelly: 100 parts thus yielded 78 of soluble matter, which, when obtained by evaporation, was of a brown resinous appearance; and, though only slightly sour to the taste, powerfully reddened the infusion of litmus.

The author remarks that the perfect solubility of this part of the galls in cold water, and its pale colour, indicate that the tannin it contains is nearly, if not perfectly, free from extractive matter; and, by pursuing the usual processes, he succeeded in obtaining it in a considerable state of purity. The tannin thus afforded is also soluble in alcohol; whence, if previous experiments be correct, it is analogous to the tannin of catechu, but distinct from that of galls, which is said to be insoluble in that menstruum.

When all soluble substances in water were removed from the Chinese galls, the residuum afforded to alcohol a minute portion of resinous matter, and 23 per cent. of insoluble woody fibre then only remained.

A further examination of the aqueous infusion proved it to contain gallic acid in considerable proportion; and the method which best succeeded in its separation, consisted in adding lime water to the cold aqueous infusion of the galls, which produces a precipitate composed of tan and lime, and leaves a gallate of lime in solution, which, when cautiously decomposed by oxalic acid, furnishes oxalate of lime and gallic acid nearly, but not perfectly, pure. The author

was equally unsuccessful in procuring that acid in a pure form by the other processes usually had recourse to. It was either combined with minute portions of tan, or, when obtained by sublimation, was empyreumatically tainted.

In conclusion, it is remarked, that the Chinese galls differ from other analogous vegetable substances in the absence of extractive matter, whence they may be regarded as the most promising source of pure tan and gallic acid; that the same circumstance renders them peculiarly fitted for the basis of a black dye, and of writing-ink, while it at the same time renders them ill calculated for the production of leather, which without extractive matter is brittle and imperfect.

Some Researches on Flame. By Sir Humphry Davy, *LL.D. F.R.S. V.P.R.I.* Read January 16, 1817. [*Phil. Trans.* 1817, *p.* 45.]

This communication is subdivided into four sections, of which the first treats of the effect of rarefactions of the air, by diminished pressure, upon flame, and explosion. An inflamed jet of hydrogen was placed in the receiver of an air-pump, and the flame was observed to enlarge during exhaustion, till the gauge indicated a pressure of one fourth or one fifth; it then diminished in size, but was not extinguished till the pressure was reduced to between one seventh and one eighth. A somewhat larger jet burned until the rarefaction amounted to one tenth, and rendered the glass tube whence the gas issued white hot. To this circumstance the author refers the long-continued combustion of the gas, and thinks the conclusion confirmed by the following experiment. A platinum wire was coiled round the jet tube, so as to reach into and above the flame, and it became white hot during the exhaustion, and continued red hot even when the pressure was only one tenth. The lower part of the flame was now extinguished, but the upper part in the contact of the wire continued to burn till the pressure was reduced to one thirteenth. The flame, therefore, of hydrogen is extinguished in rarefied atmospheres, whenever the heat it produces is insufficient to communicate visible redness to platinum wire. Sir Humphry Davy was thus led to infer, that those combustibles which require least heat for combustion would burn in rarer atmospheres than those requiring more heat; and that bodies which produce much heat in combustion would burn in rarer air than those producing little heat, and experiments are detailed proving this to be the case: thus, an inflamed jet of light carburetted hydrogen, which produces little heat in combustion, and requires a high temperature for its ignition, was extinguished whenever the pressure was below one fourth, even though the tube was furnished with a wire. Carbonic oxide burned under a pressure of one sixth; sulphuretted hydrogen of one seventh. Sulphur, which burns at a lower temperature than any other ordinary combustible, except phosphorus, had its flame maintained in an atmosphere rarefied 15 times, and phosphuretted hydrogen was inflamed when admitted into the best vacuum of an excellent air-pump.

The author next proceeds to consider the influence of rarefaction, produced by heat, upon combustion and explosion. A volume of air at 212° is expanded to 2·25 volumes. At a dull red heat its probable temperature then is 1032°, provided it expand equably for equal increments of heat.

M. Grotthus has concluded that expansion by heat destroys the explosive powers of gases, but Sir H. Davy found that two parts of oxygen and one of hydrogen expanded to 2·5, its original bulk, detonated at a red heat, and in another experiment, even at a lower temperature; whence it appears, that detonating gaseous mixtures have their inflammability rather increased than diminished, by expansion by heat. In prosecuting these inquiries, the author discovered that a mixture of oxygen and hydrogen produced water at a temperature below visible redness, and without explosion or even any luminous appearance; and at a temperature a little above the boiling point of quicksilver, charcoal converts oxygen into carbonic acid without any of the ordinary phænomena of combustion.

The third section relates to the effect of the mixture of different gases upon explosion and combustion. When 1 part of a mixture of oxygen and hydrogen, in the proportions that form water, is mixed with 8 parts of pure hydrogen, the electric spark does not inflame the mixture; and its combustion is similarly prevented by 9 parts of oxygen, 11 of nitrous oxide, 1 of carburetted hydrogen, 2 of sulphuretted hydrogen, 0·5 of olefiant gas, 2 of muriatic acid gas, and five sixths of silicated fluoric acid gas. It therefore appears that other causes, besides density and capacity for heat, interfered in these phenomena; for nitrous oxide, which is one third denser than oxygen, and which has a greater capacity for heat, has lower powers of preventing explosion; and hydrogen, though fifteen times lighter than oxygen, has a higher power of preventing explosion; and olefiant gas, in this respect, precedes the others in an infinitely higher ratio than could have been expected either from its density or capacity.

The author concludes this paper with some general observations, and practical inferences founded upon the previous detail of facts. Flame may be regarded as gaseous matter, of a temperature above that which is capable of giving to solids a white heat; for heated air, though not luminous, will communicate that high temperature to solid bodies. When we attempt to pass flame through fine wire gauze, the metal so far cools the gaseous matter that it is no longer luminous. The power of metallic and other tissues to prevent the combustion of explosive gaseous mixtures, will depend upon the heat required for their combustion, as compared with that acquired by the tissue; and the flame of those bodies which are most readily inflammable, and of those which produce most heat in combustion, will pass through a wire gauze capable of intercepting those flames that produce little heat; so that the flames of different substances will pass through wire gauze at different temperatures. For instance, a tissue that has 100 apertures in the square inch will intercept the flame of alcohol, but not that of hydrogen; and a tissue which would

not intercept an explosion from olefiant gas, would prevent it with fire-damp.

The combustibility of different gases is, to a certain extent, in direct proportion to the masses of heated matter required to inflame them. A red-hot wire, one fortieth of an inch in diameter, will not ignite olefiant gas, but it will inflame hydrogen gas; and the same wire heated white-hot, will inflame olefiant gas, but will not inflame the carburetted hydrogen of the coal-mines, which fortunately is the least combustible of the inflammable gases. The cooling power of metal, in regard to flame, is well shown by encircling a very small flame with a cold iron wire, which instantly causes its extinction. The interruption of the flame, therefore, in the author's safety-lamp, depends upon no recondite cause, but is simply referable to the cooling power of the wire-work tissue.

From the facts contained in the first part of this paper, the author conceives that the light of meteors depends not upon the ignition of inflammable gases, but upon that of solid bodies; that such is their velocity of motion, as to excite sufficient heat for their ignition by the compression even of rare air; and that the phenomena of falling stars may be explained by regarding them as small incombustible bodies moving round the earth in very excentric orbits, and becoming ignited only when they pass with immense rapidity through the upper regions of the atmosphere; while those meteors which throw down stony bodies, are similarly circumstanced, combustible masses.

Some new Experiments and Observations on the Combustion of gaseous Mixtures; with an Account of a Method of preserving a continued Light in Mixtures of inflammable Gases and Air without Flame. By Sir Humphry Davy, *LL.D. F.R.S. V.P.R.I.* Read January 23, 1817. [*Phil. Trans.* 1817, *p.* 77.]

Having shown, in a former communication, that the temperature of flame is considerably greater than that required for the ignition of solid bodies, the author thought it probable that, during the combination of certain gaseous substances, the heat evolved might be adequate to the incandescence of solid matters exposed to them, though insufficient to render the gases themselves luminous, or, in other words, to produce flame.

In a combustible mixture of coal-gas and air, the author suspended a small wire-gauze safe-lamp, in which some fine platinum wire was fixed above the flame; and when the inflammation had taken place within the cylinder of gauze, the quantity of coal-gas was increased, under the idea that the heat acquired by the mixed gas in passing through the wire gauze would prevent the excess from extinguishing the flame. When this happened, the wire of platinum continued to glow, though there was no inflamed gas in the cylinder; so that the oxygen and coal-gas in contact of the wire seemed to burn without flame, and yet produced heat enough to keep the wire ignited. This conclusion was verified by introducing a hot platinum wire into a

proper mixture of coal-gas and air. It became white hot, and continued so till the mixture had lost its inflammability. Mixtures of other inflammable gases afforded similar phenomena, and likewise several inflammable vapours, as those of ether, alcohol, oil of turpentine, and naphtha. In these experiments, platinum wire is most successfully used; for it does not tarnish, and its radiating powers are slight. Palladium answers nearly as well; but the phenomena are not witnessed when wires of silver, copper, or iron are employed. It is suggested that many theoretical views will arise from the connexion of the facts detailed in this communication with those presented to the Society in the author's former paper on flame; and practical applications may also flow from the same source. By hanging some fine platinum wire, for instance, above the wick of his safety-lamp, the coal-miner will be lighted in mixtures containing such excess of fire-damp as to be no longer explosive; and where the flame is extinguished, the metal will become sufficiently luminous to guide him, while its relative brightness in different parts of the mine will indicate the state of the air, and its fitness for respiration; for when the foul air forms two fifths of the volume of the atmosphere, the ignition of the wire ceases.

De la Structure des Vaisseaux Anglais, considérée dans ses derniers Perfectionnements. Par Charles Dupin, *Correspondant de l'Institut de France, &c. Communicated by the Right Hon. Sir* Joseph Banks, *Bart. G.C.B. P.R.S.* Read December 19, 1816. [*Phil. Trans.* 1817, *p.* 86.]

Being engaged in collecting materials for a work entitled "A Picture of Naval Architecture in the 18th and 19th Centuries," the author was induced to visit this country, with a view to become acquainted with the various innovations and improvements lately introduced here in the art of ship-building; and, in the present communication, offers some remarks upon the plans proposed by Mr. Seppings, an account of which has formerly been before the Royal Society, and is printed in their Transactions for 1814.

After giving an outline of the fundamental principles upon which Mr. Seppings's improvements in naval architecture principally depend, and dwelling especially upon the diagonal pieces of timber which he employs to strengthen the usual rectangular frame-work, the author proceeds to state that similar contrivances were long ago suggested and even practised by the French ship-builders, in order to give strength to the general fabric of their vessels. Instead of making the ceiling parallel to the exterior planks, they arranged it in the oblique direction of the diagonals of the parallelograms formed by the timber and the ceiling, in the whole of that part of the ship's sides between the orlop and limber-strake next the kelson. They then covered this ceiling with riders, as usual, and placed cross-pieces between them in the direction of the second diameter of the parallelogram. This system, however, was abandoned in the French

navy, on account of its expense, of its diminishing the capacity of the hold, and of the erroneous notion that the longitudinal length of the ship was diminished by the obliquity of the ceiling. In 1755, the Academy of Sciences rewarded M. Chauchot, a naval engineer, for the suggestion of employing oblique for transverse riders; and in 1772, M. Clairon des Lauriers employed diagonal strengtheners in the construction of the frigate l'Oiseau.

Having cited these and other instances to prove that Mr. Seppings's principle is not new, at the same time allowing that the merit of rendering its utility probable, and of overcoming many difficulties in its execution, is due to that gentleman, the author proceeds more particularly to inquire how far it contributes to strengthen the vessel, so as to enable it to oppose changes of form from the action of external powers. If every elementary part of the vessel rested immediately on the sea, it would displace its weight of water, and would only be submitted to the slight pressure of the fluid. But as only a part of the external surface of the vessel is in contact with the water, this part is called upon to support a degree of pressure of the fluid capable of counteracting the weight of the whole mass. Hence the vessel becomes convex or arched, the curve extending from the head to the stern; but as this bending is not of constant magnitude, it is evident that, in order to apportion the resistance adequately, the strength must be made greatest where there is the greatest strain. The author furnishes some new theorems for the determination of these points, and thence concludes that the point of greatest curvature lies between the quarter-deck and forecastle, across the gangways, and much nearer the head of the ship than is commonly supposed; and that the effect of the arching is to diminish the fastness of sailing, and to increase the difficulty of performing evolutions, especially with the sails. As vessels, therefore, must inevitably suffer by this effect of arching, any method of diminishing that tendency must be valuable. M. Dupin suggests a method by which it might be ascertained whether Mr. Seppings's plan is calculated to diminish the tendency of vessels to arch; upon which subject he deems Mr. Seppings's experiments, detailed in the Philosophical Transactions, as unsatisfactory. This method, however, has not hitherto been tried, and the question, consequently, cannot be decided upon. In the meantime, says the author, there is every reason to suppose that it would prove favourable to Mr. Seppings's plan.

On a new Fulminating Platinum. By Edmund Davy, *Esq. Professor of Chemistry, and Secretary to the Cork Institution. Communicated by Sir* Humphry Davy, *LL.D. F.R.S. V.P.R.I.* Read February 13, 1817. [*Phil. Trans.* 1817, *p.* 136.]

After pointing out certain analogies between gold and platinum, which rendered it probable that the latter metal would afford a fulminating compound similar to that obtained from the former, Mr.

Edmund Davy proceeds to detail the processes by which he su ceeded in procuring it.

A solution of leaf platinum in nitro-muriatic acid was evaporat to dryness, re-dissolved in water, and precipitated by sulphurett hydrogen. This hydro-sulphuret was converted into a sulphate l the action of nitrous acid. Ammonia, in slight excess, was add to the solution of this sulphate, and the precipitate so obtained boil in a solution of pure potash. It was then collected on a filter, washe and dried at 212°. This powder is of a brown colour, explodes wi a loud report when heated to about 400°, and lacerates the substan in contact with it, in the same way as fulminating gold. At a tem perature of 300° it is decomposed without explosion when in conta with mercury. It explodes by friction, but not by percussion. is tasteless; insoluble in water, soluble in sulphuric, nitric, a muriatic acids. When heated in chlorine, muriate of ammonia a muriate of platinum are produced. When heated in ammonia a in muriatic acid gas, it is decomposed; and in the latter, with near the same phenomena as in chlorine. Heated with sulphur, it affor sulphuret of platinum.

From the method pursued in obtaining this compound, the auth inferred its resemblance to fulminating gold; and on heating it i close vessels, obtained water, nitrogen, and platinum, as the resul of its decomposition. Heated with common quick-lime, it afford liquid ammonia and a little nitrogen.

Mr. Edmund Davy next proceeds to a detail of experiments ma in order to ascertain the relative proportions of the component par of this new fulminating platinum.

In these experiments, 10 grains of the powder furnished 7·3 grai of metallic platinum.

In a fourth experiment, nitrous acid was boiled to dryness up 10 grains of the powder. The dry mass heated red-hot, furnish 8·25 grains of a gray shining substance, which is a hitherto und scribed oxide of platinum, consisting of 88·3 platinum + 11·7 ox gen. From the quantity of nitrogen yielded during the decomp sition of the fulminating platinum, and from other experiments, M Davy estimates the quantity of ammonia that it contains, at 9 p cent. and gives as its component parts,

$$\left.\begin{array}{r}78{\cdot}75 \text{ platinum.}\\ 3{\cdot}75 \text{ oxygen.}\end{array}\right\} 82{\cdot}5 \text{ oxide of platinum.}$$

$$\begin{array}{r}9{\cdot}00 \text{ ammonia.}\\ 8{\cdot}50 \text{ water.}\\ \hline 100{\cdot}00\end{array}$$

This paper concludes with some general and theoretical observ tions respecting the formation and decomposition of the new fulm nating compound. When the triple sulphate of platinum and amm nia is boiled in a solution of potash, the sulphuric acid unites to th potash, a portion of the ammonia is evolved, and the remainde

entering into intimate union with the oxide of platinum, produces fulminating platinum. The theories invented by Bergmann and Berthollet, to explain the detonation of fulminating gold, are satisfactorily applicable to the phenomena presented by the compound now described, the explosive powers of which may be referred to the sudden extrication of nitrogen, ammonia, and aqueous vapour.

On the Parallax of the fixed Stars. By John Pond, *Esq., Astronomer Royal, F.R.S.* Read February 20, 1817. [*Phil. Trans.* 1817, *p.* 158.]

Dr. Brinkley, of the Observatory of Dublin, having noticed for several years past a periodical deviation of several fixed stars from their mean places, strongly indicating the existence in them of annual parallax, the author was induced to institute a series of observations upon the subject, the results of which are submitted to the Royal Society in the present communication. Being unable to devote the mural circle, erected at the Royal Observatory in 1812, entirely to this investigation, the Astronomer Royal employed two ten-feet telescopes, fixed to stone piers, and directed to the particular stars whose parallax was suspected, and furnished with micrometers for the purpose of comparing them with other stars passing through the same field. The question of parallax is, theoretically speaking, rather curious than important; but with regard to the state of practical astronomy the case is very different, and, as far as relates to the natural history of the sidereal system, it is a subject of interest to ascertain whether the distances of the nearest fixed stars can be numerically expressed from satisfactory data, or whether it be so immeasurably great as to exceed all human powers either to conceive or determine.

The principal stars observed by Dr. Brinkley were, α Lyræ, α Aquilæ, α Cygni.

The mean of forty observations of α Lyræ, made by the Astronomer Royal between June 22 and August 21, gave for the north polar distance of that star 51° 23′ 0″·278. The mean of twenty observations nearer the period of opposition gave 51° 23′ 0″·468. The mean of thirty winter observations is 51° 23′ 0″·872. The discordance, therefore, between the winter and summer observations does not exceed 0″·6, which is only one third the discordance observed by Dr. Brinkley.

With α Cygni the total discordance in favour of parallax was 0″·556; also only one third that observed by Dr. Brinkley; and with α Aquilæ it is less than 0″·5, equal only to one fourth of the discordance observed by Dr. Brinkley.

From these and other observations detailed in the paper, the Astronomer Royal observes, that in the three stars supposed by Dr. Brinkley to have the greatest parallax, the discordance between the summer and winter observations is not less than 0″·5, and scarcely exceeds 0″·75: and that although these quantities are much less

than those found by Dr. Brinkley, they appear to be equally independent of accidental error. The author, however, is not inclined to refer the cause of the differences to parallax, for they are sensibly increased by direct comparison with an opposite star, as with Capella, in which the maximum of parallax is nearly as great as in α Aquilæ; and it is very unlikely that the parallax of one star should exceed ·5″, and that of the other be an insensible quantity. Moreover, all these stars pass the meridian about the time of the winter solstice at their maximum of parallax; and in proportion as stars do not possess this property, both the Astronomer Royal and Dr. Brinkley either find a much smaller discordance, or none at all; the author accordingly is inclined to look for some explanation of the difficulties in this peculiar circumstance.

Tables are annexed to this paper, showing the method of computing the equation, or index error, applicable to the north polar distances.

Appendix to Mr. Pond's *Paper on Parallax.* Read March 13, 1817. [*Phil. Trans.* 1817, *p.* 173.]

Being induced last autumn to suspect that the discordance in favour of parallax, before met with, might arise from variation of temperature, the author endeavoured last winter to keep the interior and exterior of the Observatory of the same temperature, which the mildness of the season easily enabled him to do. From the 1st of July 1816 to March 1817, the index error of the instrument suffered no variation; and therefore the objections urged by Dr. Brinkley were done away. Under these circumstances the observations of α Lyræ, γ Draconis, α Cygni, and α Aquilæ, showed no indication of periodical variation. Whether the fixed instruments lately erected for this investigation will confirm the above result remains to be determined.

An Account of some Fossil Remains of the Rhinoceros, discovered by Mr. Whitby, *in a Cavern inclosed in the Lime-stone Rock, from which he is forming the Break-water at Plymouth. By Sir* Everard Home, *Bart. V.P.R.S.* Read February 27, 1817. [*Phil. Trans.* 1817, *p.* 176.]

Having been requested by Sir Joseph Banks carefully to preserve such organic remains as might be found in the quarries whence the supplies of limestone for the break-water at Plymouth are drawn, Mr. Whitby, who has the superintendance of the undertaking, transmitted, in November 1816, a box of fossil bones, which form the subject of the present communication.

According to Mr. Whitby's report, they were found in a cavern of the solid limestone rock, fifteen feet wide, forty-five long, and twelve deep. It was filled with clay, and the bones lay about three feet above the bottom. This cavern is about seventy feet below the

surface of the rock, and four feet above high-water mark; and although considerable pains were taken to discover its outlet, no communication of the kind was found.

Mr. Whitby mentions that caverns have been frequently discovered in this limestone, the walls of which have been encrusted with stalactitic concretions; but in the present case no such appearance was observed in any part of the cavity,—a proof, says the author, that no opening in the rock from above had ever been closed up by the process of infiltration. In further confirmation of this circumstance, Mr. Whitby has stated that in the contract for quarrying there are two prices; one for rock, and another for clay, earth, and rubbish; and that two officers constantly attend, one for the Crown and the other on the part of the contractors, who measure the contents of all caverns containing clay or other soft materials; and that these officers distinctly state that the rock surrounding the cavern was equally hard with the other parts.

All the bones discovered in this place belong to the rhinoceros, and are evidently parts of the skeletons of three different animals. They are in a most perfect state of preservation, and every part of the surface entire, to a degree very seldom observed in fossil bones; and as the teeth of the rhinoceros differ both in form and structure from those of every other known animal, there was no kind of difficulty in recognising them. Every portion of the bones also possessed some characteristic feature proving it to have belonged to the same animal. The animals to which these bones belonged seem to have been nearly of the same size, and very large, for on comparing the fossil metacarpal bone, with that of the largest rhinoceros ever seen in this country, the skeleton of which is in Mr. Brookes's collection, that of the former was $8\frac{1}{8}$ inches long and $2\frac{1}{4}$ inches broad, while in the latter the length was only $7\frac{1}{8}$ inches and the breadth $2\frac{1}{8}$ inches. This skeleton stands 5 feet 8 inches high. It deserves remark that all the bones found in this cavern belong to the same species of animal: for although great pains were taken to ascertain whether there were any others than those sent to London, none were discovered.

Some comparative chemical analyses of different fossil bones are annexed to this paper, from which it appears that those above described are remarkably free from extraneous earthy substances, and consist almost entirely of phosphate and carbonate of lime, with only slight traces of animal matter: whereas most of the fossil bones discovered in the blue clay at Brentford, and in the argillaceous limestone at Lyme in Dorsetshire, yielded considerable portions of aluminous and siliceous earth.

Description of a thermometrical Barometer for measuring Altitudes. By the Rev. Francis John Hyde Wollaston, *B.D. F.R.S.* Read March 6, 1817. [*Phil. Trans.* 1817, *p.* 183.]

The author's attention having been drawn to the variations in the heat of boiling water, as corresponding with changes in atmospherical pressure and in the height of the barometer, he was led to construct the instrument described in the present paper, for the purpose of measuring heights with greater accuracy and convenience than by the common barometer, or by the methods formerly devised by Fahrenheit and Cavallo.

The author constructed his thermometer with different scales, from an uncertainty how far their sensibility might be carried; and in one instance the bore of the tube was so minute, and the ball so large, that every degree on Fahrenheit's scale was equal to ten inches. The instrument, however, with which the greatest number of observations were made, had a scale of 3·98 inches to every degree, and each degree is divided into 100 parts upon the scale, and into 1000 by a vernier. On comparing this thermometer with a good barometer, it was found that the two instruments agreed equally well in all parts between 30·68 inches and 28·23 inches. The result was, that a difference of 1° of Fahrenheit's scale is occasioned by 0·589 inch on the barometer; 30·603 inches (corrected) on the barometer, =213°·367 on the thermometer, and 28·191 inches barometer, =209°·263 thermometer.

Having thus ascertained the delicacy and capability of the instrument, the author proceeds to describe the best mode of constructing it, which is further illustrated by an annexed drawing. To the vernier is attached a small lens of an inch focus, which, on account of the smallness of the bore of the tube, is useful in observing the height of the mercurial thread, and by having no lateral motion, confines the view to the same direction, thus preventing parallax.

The boiler of this instrument is a tin cylinder 5·5 inches deep, and 1·2 inches in diameter, with an external cylinder 1·4 inch diameter, for preventing the transmission of heat. The bottom is single, and the bulb does not dip into the water, but is exposed to the steam only; and a bell-tent protects the lamp and boiler from the wind. The lamp is filled with oil, to which a sufficient quantity of tallow is added to make it congeal at common temperatures.

Having tried threads of various thicknesses, the author advises the scale of an inch to a degree as best adapted to all ordinary purposes; for when finer than this, it is almost impossible to give such strength to the bulb as to force the column of mercury accurately to the same height on repetition of the boiling, by reason of the resistance from friction in the tube.

With an inch scale, the variations of the barometrical thermometer are to those of the common barometer as 5:3; and the sensibility of the instrument is such, that the difference of temperature required to make water boil arising from the height of a common table, is imme-

diately perceptible. After stating the methods for adjusting this thermometer for the measurement of the greatest heights, the author details some experiments upon altitudes made with an instrument, 552 parts upon the scale of which were equal to 530 feet in altitude. With this instrument boiled on the counter of a bookseller's shop in Paternoster-row, estimated between four and five feet above the foot pavement on the north side of St. Paul's Churchyard, and boiled again in the gilt gallery of the cathedral, there was a difference of 254 parts; the corrected height thus indicated therefore = 272·64 feet. General Roy makes the gallery above the north pavement to be 281 feet, which, allowing five feet for the difference of station, brings the author's estimate to 267 feet, differing only four feet; or by another calculation, founded on General Roy's statement, the difference is less than two feet.

Observations on the Analogy which subsists between the Calculus of Functions and other branches of Analysis. By Charles Babbage, *Esq. M.A. F.R.S.* Read April 17, 1817. [*Phil. Trans.* 1817, *p.* 197.]

At the commencement of this paper the author states the advantages which may be derived from the employment of analogical reasoning in mathematics, and recommends it as a very useful guide to new discoveries: he then proceeds to point out the striking resemblance which subsists between several parts of common algebra and the integral calculus, and similar parts of the calculus of functions.

Mr. Babbage then notices certain fractions which, by peculiar relations among the functions of which they consist, become evanescent. The true values of these fractions are ascertained, and they are applied to the solution of a class of functional equations which the author had solved in a former paper, from which the following result is obtained:—"Whenever the mode of solution there adopted seems to fail, the failure is *apparent* only, and the general solution may always be deduced from it."

Several points of resemblance between the integral calculus and that of functions, are then noticed; and a remarkable analogy between a method of integrating differential equations, and a mode of solving functional equations, is pointed out; in both cases the operations are performed by multiplying by a factor, whose form is to be determined by another equation. Some equations are given in which this method is successful, and the obstacles to its general application are pointed out as demanding further inquiry.

Of the Construction of Logarithmic Tables. By Thomas Knight, *Esq. Communicated by* Taylor Combe, *Esq. Sec. R.S.* Read February 27, 1817. [*Phil. Trans.* 1817, *p.* 217.]

Two General Propositions in the Method of Differences. By Thomas Knight, *Esq. Communicated by* Taylor Combe, *Esq. Sec. R.S.* Read February 27, 1817. [*Phil. Trans.* 1817, *p.* 234.]

Note respecting the Demonstration of the Binomial Theorem inserted in the last Volume of the Philosophical Transactions. By Thomas Knight, *Esq. Communicated by* Taylor Combe, *Esq. Sec. R.S.* Read April 17, 1817. [*Phil. Trans.* 1817, *p.* 245.]

In this note the author expresses his regret at finding that the demonstration of the binomial theorem, and the first proposition of his paper on the construction of logarithms, formerly presented to the Royal Society, had been previously given by Mr. Spence in his Essay on Logarithmic Transcendents. This author, however, says Mr. Knight, is not particularly happy in the manner of developing the kind of functions treated of in his preface, and therefore in the present note gives a solution of a class of equations of which Mr. Spence has considered a particular case, without however resolving.

On the Passage of the Ovum from the Ovarium to the Uterus in Women. By Sir Everard Home, *Bart. V.P.R.S.* Read May 1, 1817. [*Phil. Trans.* 1817, *p.* 252.]

No physiological subject has attracted more attention than the first formation of the embryo in the class Mammalia; and although it has been ascertained that an ovum is formed in the ovarium of the quadruped, the circumstances respecting its impregnation have not been ascertained. Harvey, and John and William Hunter, have each failed in this inquiry; Haighton and Cruikshank were equally unsuccessful. In this state of our knowledge, says Sir Everard Home, accident has led to that which no predetermined experiments could have accomplished, and has enabled me to detect the ovum in the human uterus. It is so small, that had not the uterus been previously hardened in spirit, it would probably have escaped observation; and, says the author, it would have been difficultly identified as the ovum from which a child was to be produced, had it not been for the assistance of Mr. Bauer, the only person who could so correctly apply the powers of the microscope as to enable him accurately to delineate its organization.

The history of the case and dissection is as follows.

A servant maid, twenty-one years of age, left her master's house the 7th of January, 1817, for several hours in the forenoon. On returning in the evening she complained of sickness, and went to bed. Next day she continued unwell. The period of menstruation had arrived, but did not come on. She appeared much distressed in her mind. On the 13th she had an epileptic fit, became delirious, and died on the 15th. On examining the uterus it showed signs of pregnancy; and circumstances proved that she must have been impregnated on the 7th of January, that is eight days before her death.

The right ovarium had a small torn orifice upon the most prominent part of its external surface, which led to a cavity filled up with coagulated blood, and surrounded by a yellowish organized structure. The inner surface of the uterus was covered with coagulable lymph, among the fibres of which, near the cervix, was the ovum. It was oval-shape; and though at first partly semitransparent, became opake from the action of the spirit. It was immediately taken to Mr. Bauer, who compared it to the egg of an insect, and succeeded in pointing out the effects of impregnation in two projecting points, the rudiments of the heart and brain.

The corpus luteum has always been regarded as the effect of impregnation,—a notion which the present case has enabled the author to disprove, by showing it to be a glandular structure in which the ovum is formed; and after its expulsion the blood which fills the cavity is absorbed, leaving a small empty space as the former situation of the ovum.

Sir Everard Home examined several ovaria, where it was impossible that impregnation should ever have taken place, and found small cavities round the edge of the ovarium, showing that during the state of virginity ova had passed out. And it appears, that whenever a female quadruped is in heat, one or more ova pass into the uterus, whether she receives the male or not.

In the drawings belonging to this paper, the changes which take place in the ovarium, for the purpose of forming the ova, are shown, and also the internal surface of the Fallopian tube at the time of the passage of the ovum. The dilatation of this tube at a small distance from the fimbriæ, seems to be both for the reception of the ovum and of the semen; and it is probable that the ovum is retained there for several days, so as to prolong the opportunity of its being impregnated.

The formation of ova in the ovaria, and their appearing in that organ in succession, induces the author to entertain an opinion contrary to that commonly received respecting menstruation, which has been considered as a necessary preparatory step for utero-gestation, whereas the present case shows that such periods are not connected with the formation of the ovum, the process of its leaving the ovarium, or its impregnation. When, however, impregnation does not take place, such a discharge seems necessary for the relief of parts to which there had been so copious a determination of blood.

The paper concludes with Mr. Bauer's account of the appearance of the ovum, and of the drawings which are annexed to the paper.

Some farther Observations on the Use of the Colchicum autumnale *in Gout. By Sir* Everard Home, *Bart. V.P.R.S.* Read May 8, 1817 [*Phil. Trans.* 1817, *p.* 262.]

When the infusion of colchicum is kept for some time, it throws down a sediment, in which the purgative qualities of the root appear

principally to reside, while its separation does not seem to diminish the specific effects of the medicine upon gout.

When the bulb of the *Colchicum autumnale*, says the author, is infused in wine, both extractive matter and mucilage are taken up, but a copious deposition takes place in the strained tincture. Several experiments proved that this first deposit is nearly inert; but the clear liquor subsequently forms other depositions, and it is to these that the experiments in this paper relate. The author took half a bottle of *Eau medicinale* containing none of the sediment, and its effects were extremely mild compared with those of the other half containing the sediment.

To ascertain how far this sediment, thus shown to contribute to the violent operation of the medicine upon, was active in curing the gout, Sir Everard made the following experiment. Sixty drops of the clear vinous infusion were given to a man labouring under a severe fit of gout. It produced slight nausea, and operated twice gently upon the bowels. In 19 hours his pulse fell from 115 to 92, and in 48 hours he was well, and continued so for more than three months.

The result of this case satisfied the author that the principle in the colchicum which cures gout is retained in permanent solution, and induced him to believe that the violent effects of the remedy upon the stomach and bowels, must depend upon some distinct substance contained in the sediment.

To ascertain this point several experiments were instituted upon dogs, tending not merely to confirm Sir Everard's opinion upon this head, but likewise satisfactorily to demonstrate the identity of the *Eau medicinale* with the infusion of *Colchicum autumnale*. The experiments were made by Mr. Gatecombe, with the following results. Thirty drops of *Eau medicinale*, with the deposit injected into the jugular vein, produced a purging of nine hours duration. One hundred and sixty drops administered in the same way, killed the dog in six hours, and there were appearances of violent inflammation in the bowels. The same quantity taken by the mouth, produced nearly similar effects. One hundred and sixty drops of the vinous infusion of colchicum, without sediment, produced purging and vomiting, from which the animal recovered. Two hundred drops produced the same effect, but 300 drops produced effects corresponding with those of the 160 of *Eau medicinale*. The dog died in nine hours. Hence it appears that the *Eau medicinale* produces double the effect of the vinous infusion of colchicum. Six grains of the deposit from the vinous infusion, produced vomiting and purging, with blood, which lasted twenty-four hours.

Sir Everard conceives, from these experiments, that the acrimonious and highly drastic effect of the *Eau medicinale*, is to be referred to the sediment which it deposits, and notices the analogy which thus exists between it and the juice of the wild cucumber, the sediment of which is known under the name of *Elaterium* as a highly drastic purge, whereas the juice which has deposited this substance is comparatively inert.

Upon the Extent of the Expansion and Contraction of Timber in different directions relative to the Position of the Medulla of the Tree. By Thomas Andrew Knight, *Esq. F.R.S. In a Letter addressed to the Right Hon. Sir* Joseph Banks, *Bart. G.C.B. P.R.S.* Read May 8, 1817. [*Phil. Trans.* 1817, *p.* 269.]

Most of the attempts which have been made by writers on vegetable physiology, to account for the force with which the sap of trees ascends during the spring, having proved unsatisfactory and inadequate, Mr. Knight was induced some years ago to suggest the expansion and contraction of the cellular processes proceeding from the bark to the medulla, and which he called the true or silver grain of the wood, as concerned in this process.

The present paper contains further experiments, showing this power to be active in living trees, and were made on many kinds of timber with nearly similar results. Some boards of ash and beech wood were cut in opposite directions relative to their medulla, so that the convergent cellular processes crossed the surfaces of some of them at right angles, and were parallel with the surfaces of others. These were placed, under similar circumstances, in a warm room, and the former warped about ten times more than the latter, contracting nearly 14 per cent. in breadth, while the others only contracted $3\frac{1}{2}$ per cent. During his experiment Mr. Knight was led to infer that the medullary canal must be liable to considerable changes of diameter, as the moisture of wood increases or diminishes. To ascertain this, parts of the stems of young trees were carefully dried, the medulla was removed, and metal cylinders driven with force into the empty space. The pieces of wood were then suffered to absorb moisture, and the medullary canal became so much enlarged as to suffer the cylinders to fall out.

Mr. Knight conceives that this kind of expansion often produces those rifts in trees referred to wind or frost. That winds cannot be the cause, seems obvious from the circumstance of pollard-oak-trees being almost always rifted, upon which they can have little power; and the frost of this climate is seldom sufficiently intense to congeal the winter sap in trees.

The force with which this cellular substance of timber expands, is more than adequate to such effects, and often overcomes a pressure of many tons; and as it is in action in the living tree, Mr. Knight is of opinion that it is the agent by which the powerful propulsion of the sap observed by Hales is effected.

Observations on the Temperature of the Ocean and Atmosphere, and on the Density of Sea-water, made during a Voyage to Ceylon. In a Letter to Sir Humphry Davy, *LL.D. F.R.S. By* John Davy, *M.D. F.R.S.* Read May 22, 1817. [*Phil. Trans.* 1817, *p.* 275.]

The experiments on the specific gravity of sea-water, detailed in this paper, were partly conducted at sea, and partly after the author's

arrival at Ceylon; and the results lead him to adopt the opinion that the ocean resembles the atmosphere in being, *cæteris paribus*, nearly of the same specific gravity throughout. The water used in the experiment was always taken from the surface of the ocean. The variation of specific gravity was most observable when the water was rough and agitated, and seemed in one instance diminished by heavy rain.

Dr. Davy doubts whether a modern traveller of high authority is correct in supposing that a peculiar specific gravity belongs to the water of each zone; for in his own experiments, the water taken in latitude 0° 12′ S., and 22° 36′ S., was of similar specific gravity, as also that taken at 34° 25′ S., and that washing the shores of Columbo.

The trials of the temperature of the air and water were, during the greater part of the voyage, made every two hours, night and day. The variations of atmospheric temperature, says the author, follow the course of the sun. They are pretty considerable whilst he is above the horizon, and very insignificant during the night. At a great distance from land, and with a steady wind between and bordering upon the tropics, the diurnal variation of atmospheric temperature appeared perfectly regular; its maximum precisely at noon, its minimum towards sunrise. In a calm, the maximum of heat was some time after noon, and the regular law of variation is more obviously interfered with by storms and rain.

The temperature of the sea was found liable to variations nearly as great as those of the incumbent atmosphere. In fine quiet weather, at a great distance from land, the maximum of temperature was about 3 P.M., and the minimum towards sunrise. It is, however, subject to irregularities. In tempestuous weather superficial currents seem to be established in the direction of the prevailing winds, which increase or lower the temperature according as the wind is hot or cold. Where the sea is shallow, its temperature is comparatively low; a fact which may sometimes prove useful in indicating to the mariner the vicinity of shallows. In approaching the Cape of Good Hope and Ceylon, the author had occasion to observe this fact: in the latter case there was a reduction of 2° on coming into soundings. In considering the effects of currents upon the temperature of the sea, Dr. Davy particularly notices that which flows round the bank of Lagullas from the S.E. coast of Africa, and which is 10° above the surrounding sea; a difference partly referable to the banks which border the current. The dense mist which occasionally covers the Table Mountain is considered by Dr. Davy as connected with this current, and produced by the condensation of the vapour rising from this current by a cold S.E. breeze, during which the phenomenon only happens.

This communication concludes with some general and practical inferences connected with the use of the thermometer at sea: it contains several tables of results, and of meteorological observations.

Observations on the Genus Ocythoë *of* Rafinesque, *with a Description of a new Species. By* William Elford Leach, *M.D. F.R.S.* Read June 5, 1817. [*Phil. Trans.* 1817, *p.* 293.]

Several ancient and modern writers have described a species of Ocythoë often found in the Paper Nautilus, and have considered it as belonging to that shell. Sir Joseph Banks and other naturalists have maintained a contrary opinion, and have considered the Ocythoë as a parasitical inhabitant of the Argonaut's shell. Rafinesque, whose opportunities for observation were commensurate with his talent in observing, regarded it as a peculiar genus, allied to the *Sepia octopodia* of Linnæus, and as a parasitical resident of the above-mentioned shell.

The observations of the late Mr. John Cranch, zoologist to the Congo expedition, have, in the opinion of Dr. Leach, removed all doubt upon this subject. In the Gulf of Guinea he took several specimens of a new species of Ocythoë in a small Argonauta, and placed two of them in a vessel of sea-water, so as to observe their motions. When adhering to the basin the shell could be removed; they had the power both of retiring within it and of entirely quitting it. One having left the shell lived several hours, and showed no desire to return. Others quitted the shell while taking up the net. Ocythoë differs from the Polypus in the shortness of its arms; in having pedunculated instead of simple suckers; in having four oblong spots on the inside of the tube, and a small fleshy short tubercle immediately above the bronchiæ, on each side,—a character common to this genus, to Loligo, and to Sepia, but which does not exist in Polypus.

This paper concludes with a descriptive reference to the drawing, which shows the animal in and out of the shell. It is called by the author *Ocythoë Cranchii*.

The distinguishing Characters between the Ova of the Sepia, and those of the Vermes Testacea, that live in Water, explained. By Sir Everard Home, *Bart. V.P.R.S.* Read June 5, 1817. [*Phil. Trans.* 1817, *p.* 297.]

After alluding to the erroneous notions of Linnæus and other naturalists, concerning the animal that forms the shell called Argonauta, and to his own opinion that it is an internal shell, the author proceeds to show that this shell is not the produce of the species of Sepia often found in it, for the ova of this Sepia are not those of an animal of the Order Vermes Testacea. The blood of oviparous animals, while in the egg, is aërated through its coats; but in the Vermes Testacea, if the shell were formed in the egg, the process of aëration would be impeded; so that the animal's shell is formed after it has left the egg. Animals that live in water require some defence while the shell is forming; they are therefore inclosed in a camerated nidus. That of the *Helix ianthina*, taken in the voyage to the Congo,

is described by Sir Everard Home. The ova are deposited upon its own shell; sometimes one only, sometimes several, contained in one chamber.

The animal found in the Argonaut shell by Mr. Cranch, had deposited eggs upon the lip of the shell; they were united by pedicles, like those of the *Sepia octopus*, and differed from those of the *Helix ianthina*, and other testaceous Vermes living in water, in having no camerated nidus, and in having a very large yolk to supply nourishment to the young animal when hatched; so that this animal, says Sir Everard Home, must be resolved into a species of Sepia; an animal which has no external shell, and which only uses the Argonaut when it occasionally gets possession of one.

Some naturalists not acquainted with comparative anatomy have thought they saw the Argonauta shell partly formed in these ova. The appearance they allude to is probably the unusually large yolk.

Astronomical Observations and Experiments tending to investigate the local Arrangement of the Celestial Bodies in Space, and to determine the Extent and Condition of the Milky Way. By Sir William Herschel, *Knt. Guelp. LL.D. F.R.S.* Read June 19, 1817. [*Phil. Trans.* 1817, *p.* 302.]

The construction of the heavens, in which the real place of every celestial object in space is to be determined, can only be delineated with precision when we have the situation of each heavenly body assigned in three dimensions, which, says the author, in the case of the visible universe, may be called longitude, latitude, and profundity. The angular positions of the stars given in astronomical catalogues, and on globes and maps, may enable us to find them by the eye or telescope; but their distance remains unknown; and unless a proper method for obtaining the profundity of objects can be found, their longitude and latitude will not enable us to assign their local arrangement in space. The method of parallaxes has succeeded with regard to objects comparatively near. The parallax of the fixed stars has also been an object of attention; and although the investigation has hitherto produced nothing satisfactory, it has given us a magnificent idea of the vast extent of the sidereal heavens, by showing that probably the whole diameter of the earth's orbit, at the distance of a star of the first magnitude, does not subtend an angle of more than a single second of a degree. To stars of a smaller size the parallactic method admits of no application.

Sir William Herschel proceeds to consider the local situations of the stars, and proposes a standard by which their relative arrangement may be examined; that is, by comparing their distribution to a certain properly modified equality of scattering, in which it is not required either that the stars should be equidistant from each other, or that those of the same nominal magnitude should be equally distant from us. A certain equal portion of space is allotted to every star, so that we may thus calculate how many stars any given ex-

tent of space should contain. This arrangement is further explained by reference to an annexed diagram.

Sir William Herschel next compares the order of magnitudes with the order of distances, the result of which is, that if the order of magnitudes could indicate the distance of the stars, it would denote at first a gradual, and then a very abrupt condensation of them; but that, considering the principle upon which the stars are classed, their arrangement into magnitudes can only apply to certain relative distances; and show that, taking the stars of each class one with another, those of the succeeding magnitudes are further from us than the stars of the preceding order.

In the fourth and fifth sections of this paper, the means of ascertaining the profundity or local situation of the celestial objects in space, and the equalization of star light are discussed; and these are succeeded by a series of observations on the extent of natural and telescopic vision, and their application to the probable arrangement of the heavenly bodies in space.

This paper concludes with a series of observations on the extent and construction of the Milky Way; which, with his former observations, the author is inclined to think will contain nearly all the general knowledge we can ever have of this magnificent collection of stars.

Some Account of the Nests of the Java Swallow, and of the Glands that secrete the Mucus of which they are composed. By Sir Everard Home, *Bart. V.P.R.S.* Read June 26, 1817. [*Phil. Trans.* 1817, *p.* 332.]

After noticing the various opinions which have been entertained concerning the materials of which the nests of the Java Swallow are composed, Sir Everard proceeds to examine the glandular structure of its œsophagus and stomach, as also the chemical characters of the substance forming the nest. He was chiefly led to this inquiry by the suggestion of Mr. Raffles, who was of opinion that the matter of the nest was brought up from the bird's stomach, and that the violence of the effort was occasionally such as to be accompanied with blood, the stain of which is sometimes seen on the nests.

On examining the gastric glands of the Java swallow with a common magnifying glass, their orifices obviously differed from those of other birds, and of the common migrating swallow of England.

The Java swallow is a constant inhabitant of the caverns of that island, and about twice the size of the common swallow. It builds two nests; one oblong and narrow for the male, the other wide and deep for the female and her eggs. The peculiarity of structure in the gastric glands consists in a membranous tube surrounding each of their ducts, which, after projecting into the gullet, splits into separate portions, like the petals of a flower. From the surface of these tubes the peculiar mucus for the formation of the nest is secreted. Thus, says the author, the opinion which I have long

adopted, "that membranes upon which no glandular structure can be discovered are capable of secreting mucus," is confirmed.

From the annexed chemical examination of the Javanese swallow's nest, it appears to possess a close analogy to albumen; differing from the ordinary properties of that principle in being easily soluble in liquid ammonia, and in the solution of its subcarbonate, and in affording a relatively smaller proportion of ammoniacal products when submitted to destructive distillation.

This paper is accompanied by a drawing, exhibiting magnified representations of the gastric glands in the blackbird, and in the common and Java swallow; thus rendering the differences of structure, so as to preclude the necessity of any extensive details.

Observations on the Hirudo complanata, *and* Hirudo stagnalis, *now formed into a distinct Genus under the name*, Glossopora. *By Dr.* Johnson, *of Bristol. Communicated by Sir* Everard Home, *Bart. V.P.R.S.* Read June 26, 1817. [*Phil. Trans.* 1817, *p.* 339.]

The animals named in the title of this paper differ so considerably from the Leech, as to induce the author to remove them from the genus Hirudo, and to form them into a distinct one under the term Glossopora, a term derived from a prominent feature of the animal, namely, its projectile tubular tongue.

They resemble the leech, in the body being furnished with a series of rings, in locomotion being effected by the alternate motion of the head and tail, and in the division of one general stomach into several lateral cells or partitions. They differ from the leech in the mouth being furnished with a projectile tubular tongue; in the flat pyriform shape of the body; and in having an abdominal pouch or cavity for the reception of their young. After enumerating the character of the genus, Dr. Johnson expresses his opinion that the *Hirudo circulans*, *Hirudo crenata*, *Hirudo hyalina*, and *Hirudo tessulata*, will be found to belong to it; and the *Hirudo sexoculata*, described by Bergmann in the Stockholm Transactions, seems to be the same animal. Its tongue is cartilaginous, flexible, and about one eighth of an inch long. The author describes the *Glossopora tuberculata* and the *Glossopora punctata*. The notion that they are capable of reproduction when cut or divided, he considers without foundation. Their food consists chiefly of water Helices; into the shell of which they easily penetrate in consequence of their tapering head, and from the flexibility of the tongue they are enabled to follow their victim to the innermost recess of its habitation. The ova are received into the abdominal pouch of the parent, where they remain till fully evolved, and they are unproductive if moved from this situation.

An annexed drawing illustrates the anatomy and habits of these animals.

Observations on the Gastric Glands of the human Stomach, and the Contraction which takes place in that Viscus. By Sir Everard Home, *Bart. V.P.R.S.* Read June 26, 1817. [*Phil. Trans.* 1817, *p.* 347.]

This paper contains an account of the internal membrane of the human stomach, in reference to magnified views of the different structures composing its surface, executed by Mr. Bauer.

The œsophageal glands have the appearance of infundibular cells. The structure upon the upper arch of the stomach is made up of cells, of the form of a honeycomb; and this structure extends, though less visibly, over the whole surface of the cardiac portion. In the pyloric portion the cells have the same appearance; but there are small clusters, the sides of which rise above the surface, giving the appearance of foliated membranes.

Having formerly shown that the gastric glands are largest and most numerous in the animals that inhabit the least fertile regions of the earth, and *vice versâ*, the author remarks the greater necessity for the same arrangements in man, whose gastric glands are so small as to require microscopic aid to prove that they appertain to the same series of structures as those of the ostrich, which may be minutely examined by the unaided eye.

Sir Everard alludes to his former discovery of the occasional division of the stomach into two portions by a muscular contraction, which he is now able further to elucidate by a case in which this contraction had become permanent, and which probably caused the death of the woman in whom it occurred. The importance of this fact in studying the physiology of the stomach, is the only apology, says the author, which I shall make for having pressed it so much on the attention of the Society: its use in the pathology of that viscus, though perhaps of still more importance to the cause of suffering humanity, this is not the proper place to consider.

A drawing of the contracted stomach also is annexed to this paper.

On the Parallax of the fixed Stars. By John Pond, *Esq. Astronomer Royal.* Read June 26, 1817. [*Phil. Trans.* 1817, *p.* 353.]

The object of this paper is to communicate a series of observations made with a new instrument for the purpose of investigating the question of parallax. Though a much longer period of time will be necessary to elucidate this subject in a perfectly satisfactory manner, yet, from the observations already made, it seems highly probable that the parallax of α Cygni is too small a quantity to have had any share in producing either the discordances remarked by Dr. Brinkley, or those in the Greenwich observations already communicated to the Society.

The method consists in continually observing the meridional difference in polar distance of α Cygni and β Aurigæ, (which pass through the field of the same telescope,) by means of a micrometer adapted to this purpose.

A drawing of the instrument accompanies the observations.

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A drawing of the instrument accompanies the observations.

On the great Strength given to Ships of War by the application of Diagonal Braces. By Robert Seppings, *Esq. F.R.S.* Read November 27, 1817. [*Phil. Trans.* 1818, *p.* 1.]

The principle of applying diagonal frame-work to ships of war was first partially and successfully adopted in the Kent, of 74 guns, in the year 1805, and since that period has been successfully employed in the construction of thirty-eight sail of the line and thirty frigates. These circumstances might be deemed conclusive as to the advantages of the new system; but as the Royal Society have already published this author's account of it at a very early period of its adoption, he is induced to offer the result of a new experiment in proof of the correctness of the principles before laid down, which, as far as his knowledge extends, has never been previously applied, nor ever suggested by any continental writer, though, says the author, it has been pretty broadly insinuated that the hint was borrowed from the French.

In the early part of the present year, the Justitia, an old 74, was ordered to be broken up; when Mr. Seppings, notwithstanding her shattered condition, determined to apply the trussing principle. Prior to her being taken into dock, sights were placed in the lower and upper gun-deck, to ascertain, when she had grounded on the blocks, how much she deviated from her state afloat. She was then partially trussed, as described by reference to an annexed drawing, and floated out into the basin. After lying one hour, it was found, by the sights placed on the gun-deck, that she had come down in the mid-ship 1 foot; and by those on the upper-deck, 1 foot $2\frac{3}{4}$ inches. In twenty-four hours she further hogged $2\frac{1}{4}$ inches, and then appeared stationary. The trusses in the hold were then removed, and she further hogged 6 inches, and $3\frac{1}{4}$ inches in removing those in the ports.

In further illustration of the efficiency of the principle, Mr. Seppings adduces the Nelson, St. Vincent, and Howe, three 120-gun ships of the same dimensions; the two former built upon the old plan, the latter upon the diagonal system. The Nelson, after she was launched, altered $9\frac{1}{4}$ inches from the original sheer, the St. Vincent $9\frac{1}{4}$, and the Howe only $3\frac{3}{8}$ inches.

The paper concludes with a very favourable report from Captain Coode, of the state of His Majesty's ship the Albion, after the memorable battle of Algiers, which, in his opinion, stood the concussion from the firing better than would have been the case had she not been constructed on the diagonal plan; and with an account of the deck of the Northumberland, which was laid on one side fore and aft as usual, and on the other side diagonally, the materials on each side being similar. After her return from St. Helena, the officers of the Sheerness yard, who were directed to examine her, reported, that having examined the state of the decks and waterways, they found the comparison so much in favour of the larboard side, as to determine in favour of the diagonal system.

A Memoir on the Geography of the North-eastern part of Asia, and on the Question whether Asia and America are contiguous, or are separated by the Sea. By Captain James Burney, *F.R.S.* Read December 11, 1817. [*Phil. Trans.* 1818, *p.* 9.]

The opinion that the continents of Asia and America are separated by the sea, seems first to have been inferred in 1736 by Professor Müller, and to have been founded upon some papers found at that time in Siberia, relating to the celebrated voyage of Deschneer, who in 1648 first discovered the sea east of Kamtschatka. The question, however, seems to have been undecided at the period of Behring's voyage, as we learn from his instructions given by Peter the Great; and the Asiatic side only of Behring's Strait was discovered by that navigator: for the coast of Asia being there found to take a western direction, it had the effect of giving an impression of the total separation of Asia and America. After noticing several other attempts to determine the north-eastern limits of Asia previous to the arrival of Captain Cook in the sea of Kamtschatka, Captain Burney proceeds to the observations of that navigator.

The first extraordinary circumstance was a sudden disappearance of the tides in Behring's Strait. Thence Captain Cook coasted the land of America to the north and north-east till stopped, in 70° 40′ north latitude, by a floating body of ice. He then stood westward from the coast of Asia, keeping in as high a latitude as the ice would permit. The deepest soundings in this sea did not exceed 30 fathoms in latitude 68° 45′, mid-way between the coasts of Asia and America. The soundings decreased to the northward, and did not increase in running from the coast of America westward, as is usual in running from land. These, and other peculiarities, gave so much the character of a mediterranean sea, that some on board, says the author, were of opinion that we were inclosed by land to the north, and that Asia and America were there united.

Captain Burney next notices the expedition of Commodore Billings; in which, however, no new facts respecting the main question were discovered.

In this uncertainty respecting the north-east termination of Asia, the author gives one observation of importance,—which is, that the Tschuktzki people do not appear to know the extent of their country north, nor to give any satisfactory information respecting it, though some of them have travelled from the continent to islands in the Icy Sea. The most probable chance of arriving at any certainty upon the subject of the north-eastern boundary of Asia, is that, says the author, which was recommended by the Russian Admiralty to Commodore Billings, *i. e.* to trace the coast in sledges where the sea is frozen.

The principal argument against the probability of Asia and America being joined, is, that the northern land in the Icy Sea has repeatedly been supposed and reported to be an extension of the American continent, and not to join the Tschuktzki country.

In Captain Krusenstern's Memoir on the Lands of the Icy Sea, it is related that 250 versts of the coast of a northern land was very lately explored, which has been called New Siberia; and at the easternmost part of this land the coast took a direction to the north-west, which appeared to render it not probable that it joined the Tschuktzki land: but nevertheless, the coast, in Captain Burney's opinion, may turn to the east; and the Russian discoverer Hederstroom considers that this is the case, and that New Siberia is a prolongation of America. The Tschuktzki people, says the author, would not explore further north than afforded a prospect of reward for their pains, which has led them to some of the islands of the Icy Sea, though there is no evidence of their having yet reached New Siberia. On the whole, Captain Burney is of opinion that Asia and America are part of one and the same continent.

Additional Facts respecting the Fossil Remains of an Animal, on the subject of which two Papers have been printed in the Philosophical Transactions, showing that the Bones of the Sternum resemble those of the Ornithorhynchus paradoxus. *By Sir* Everard Home, *Bart. V.P.R.S.* Read January 22, 1818. [*Phil. Trans.* 1818, *p.* 24.]

In an engraving annexed to Sir Everard Home's first paper upon the above subject, a portion of bone is shown lying upon the scapula, which he considered as a portion of a rib accidentally brought there; but which he now finds to be nearly in its original situation, and is found to resemble nearly the clavicular bone in birds, as far as regards relative position.

The bones of the sternum were first pointed out to the author by Mr. Buckland; and their discovery destroys the analogy between this fossil animal and cartilaginous fishes. On comparing the general form of the sternum with that of the *Ornithorhynchus paradoxus*, a general agreement was discovered between them: they differ in the fossil skeleton having a clavicular bone, which is wanting in the other, and in the Ornithorhynchus having a long process from the scapula, which the fossil bone wants.

The fossil animal is ascertained to have lived in water, by the form of its vertebræ; and from the shape of the chest, it must have breathed air; in these respects resembling the Ornithorhynchus: but the mode of progressive motion differs: that of the one being the same as in fishes, that of the other the same as in the whale tribe.

Another bone is described in this paper, probably belonging to the same animal, and which the author regards as the first bone of the pectoral fin; which, however, cannot be absolutely determined till the bones of the pelvis are found.

To find any analogy, says the author, between the bones of animals now alive and those of races long extinct, is matter of no small curiosity; but to have discovered an analogy between the peculiarities belonging to the animals of New Holland, by which they are so remarkably distinguished from all others that now inhabit our globe,

and bones in a fossil state, creates a considerable degree of surprise; and by connecting the present animals with those that are extinct, adds a link to that chain of gradation which is the most interesting to the comparative anatomist and to the geologist.

An Account of Experiments for determining the Length of the Pendulum vibrating Seconds in the Latitude of London. By Capt. Henry Kater, *F.R.S.* Read January 29, 1818. [*Phil. Trans.* 1818, *p.* 33.]

It has long been a desideratum in science, to determine the precise length of a pendulum vibrating seconds in a given latitude. Most of those who have undertaken this inquiry have endeavoured to find the centre of oscillation; but as this depends upon the regular figure and uniform density of the body employed, it involves difficulties which may be considered as insurmountable. Despairing, therefore, of success in any attempt founded upon such principle, Captain Kater endeavoured to discover some other property of the pendulum less liable to objections; and was so fortunate as to perceive one which promised an unexceptionable result.

It is known that the centres of suspension and oscillation are reciprocal; or, in other words, if a body be suspended by its centre of oscillation, its former point of suspension then becomes the centre of oscillation, and the vibrations in both positions will be performed in equal times. Now as the distance of the centre of oscillation from the point of suspension depends upon the figure of the body employed, if the arrangement of its particles be changed, the place of the centre of oscillation will also suffer a change. Suppose, then, a body to be furnished with a point of suspension, and another point on which it may vibrate, to be fixed as nearly as can be estimated in the centre of oscillation, and in a line with the point of suspension and centre of gravity; if the vibrations in each position should not be equal in equal times, they may readily be made so, by shifting a moveable weight, with which the body is to be furnished, in a line between the centres of suspension and oscillation; when the distance between the two points about which the vibrations were performed, the length of a simple pendulum, and the time of its vibrations, will at once be known, uninfluenced by any irregularity of density or of figure. The mode of suspension which the author adopted was the knife-edge, of which the various advantages and disadvantages are pointed out, and the modes of overcoming the latter described.

The pendulum consisted of a thin bar of plate-brass, pierced with two triangular holes at the distance of 39·4 inches from each other to admit the knife-edges, which were made of wootz, and finished to an angle of 120°, and firmly screwed to brass knee-pieces. The pendulum is prolonged at either extremity by a slip of deal, extending about twenty-two inches beyond the knife-edges. Three weights are employed for the adjustments. The great weight is immoveably fixed beyond the knife-edges; the second weight slides on the bar,

near the knife-edge, at the opposite end, and may be fixed at pleasure; the third weight is a small slider, intended to move near the centre of the bar, upon which are engraved divisions of one twentieth of an inch, seen through an opening in the slider. The support of the pendulum consisted of agate planes bedded in bell-metal.

In proceeding to the details of the experiments, the author acknowledges his obligations to Henry Browne, Esq. F.R.S., who permitted him to use his house in Portland-place, and his excellent clocks, for the purposes of the investigation. The greatest daily variation of the clock used as a standard of comparison did not exceed three tenths of a second between the months of February and July.

By the method of coincidences which Captain Kater employed, the number of vibrations made by the pendulum in twenty-four hours might be obtained in the space of eight minutes to within half a second of the truth; and the usual correction was applied for the extent of the arc of vibration.

The pendulum being suspended with the great weight above, the number of vibrations in twenty-four hours was determined; and if it differed when the pendulum was inverted, it was equalized by moving the second weight, and finally adjusted by the slider, every allowance being made for the temperature, and the height of the barometer being noted. Thus the number of vibrations in twenty-four hours, of a pendulum equal in length to the distance between the knife-edges at a given temperature and barometrical height, was ascertained.

The next sections of Captain Kater's communication refer to the apparatus and methods employed for the measurement of the distance between the knife-edges; for the comparison of the British standard measures of the highest authority; and to the expansion of the pendulum, which was found to be ·00000996 of its length for each degree of Fahrenheit's thermometer.

After describing the methods of deducing the length of the pendulum vibrating seconds, and the corrections for the buoyancy of the atmosphere, the author makes it appear, that the distance of the knife-edges, at the temperature of 62° Fahr., by the mean of three several sets of measurements, the greatest difference between any two of which did not amount to $\frac{1}{10,000}$th of an inch, was, upon Sir George Shuckburgh's scale, 39·44085 inches. From a table inserted in this paper of twelve sets of experiments, each set consisting of four, from which, and from the preceding measurements, the length of the seconds' pendulum *in vacuo* is calculated, it appears that seven of these sets are within $\frac{1}{10,000}$th of an inch of the mean result; two a little exceeding $\frac{1}{10,000}$th of an inch; and of the remaining three, the greatest difference is less than $\frac{2}{10,000}$th of an inch; so that the mean result must, it is presumed, be very near the truth.

To the length thus found, the author next applies a correction for the height of the place of observation above the level of the sea. The advantages of his different methods are then explained; and the conclusion of the whole is, that the length of the pendulum vibrating

seconds *in vacuo*, at the level of the sea, measured at the temperature of 62° Fahr., and the latitude of the place of observation, deduced from the data contained in the trigonometrical survey, being 51° 31′ 8″·4 N., is,

	Inches.
By Sir George Shuckburgh's Standard....	39·13860
By General Roy's Scale................	39·13717
By Bird's Parliamentary Standard........	39·13843

On the Length of the French Mètre estimated in parts of the English Standard. By Capt. Henry Kater, *F.R.S.* Read February 5, 1818. [*Phil. Trans.* 1818, *p.* 103.]

One of the objects of the Committee of the Royal Society appointed for the purpose of determining the length of the seconds' pendulum having been to compare the French Metre with the British Standard Measure, two metres were procured from Paris for that purpose; one called the *Mètre à Bouts*, being a bar of platinum, of which the terminating planes are supposed to be parallel, and the distance between them the length of the metre; the other termed the *Mètre à Traits*, consisting also of a bar of platinum, but upon which the length of the metre is shown by two very fine lines.

The latter was first examined, by placing it in contact with Sir George Shuckburgh's standard scale; their surfaces being in the same plane, and care being taken that their temperatures were alike. The same micrometer microscopes employed in the pendulum experiments were used, and were brought alternately over the metre and over the scale. It appeared from the mean result, properly corrected, of fourteen comparisons, the greatest difference between any one of which and the mean result is less than $\frac{1}{10,000}$ths of an inch, that the length of the *Mètre à Traits*, in inches of Sir George Shuckburgh's scale, is 39·37076 inches. The author next describes the means resorted to for ascertaining the length of the *Mètre à Bouts*; which appears, from the results of four sets of experiments, each set consisting of five, the greatest difference between any one of which and the mean result is $\frac{1}{10,000}$th of an inch, to be 39·37081 inches of Sir George Shuckburgh's standard.

After explaining the principles upon which the column in the tables intitled "Correction for Temperature" is constructed, Captain Kater remarks, that we may consider the mean derived from both metres, viz. 39·37079 inches of Sir George Shuckburgh's scale, or 39·37062 inches of Bird's parliamentary standard, as the length of the French metre.

A few Facts relative to the Colouring Matters of some Vegetables. *By* James Smithson, *Esq. F.R.S.* Read December 18, 1817. [*Phil. Trans.* 1818, *p.* 110.]

The author offers the scattered facts contained in this paper to the notice of the Society, in the hope that they may induce some other person to extend the experiments, interesting not merely in chemistry but also in the art of dyeing.

The author observes that Fourcroy's opinion,—that turnsole is red originally, and made blue by carbonate of soda,—is erroneous, for its tinctures contain no alkali of any kind; he found in it a small portion of carbonate of lime. The insoluble part of turnsole is rendered red by acids, but not affected by carbonate of soda; when burned, a portion of smalt remains. The soluble part was obtained by evaporating its aqueous solution. When burned it leaves a little potash, which the author thinks essential to its composition, and that, like ulmin, it may be a compound of a vegetable principle with potash. The next colouring principle noticed by the author is that of the violet, it is reddened by acids, and becomes first green and then yellow by the alkalies and the carbonates. A similar principle exists in the petals of the red rose, of red clover, of the tips of the daisy, in the blue hyacinth, hollyhock and lavender, in the inner leaves of the artichoke, in the skin of plums, and in several other vegetable substances, also in the red cabbage. To this principle the author applies the name of Ajax, whose blood is fabled to have dyed the violet.

In sugar-loaf paper the author found two colouring matters: one red, and soluble in water; the other blue, and requiring an acid for its extraction.

The juice of the black mulberry is rendered green by caustic potash, blue by carbonate of soda, and vinous red by carbonate of ammonia. When mixed with chalk it rendered that substance blue, and the filtered liquor was red, and could not be made blue by further addition of the chalk. Heat did not affect the red colour of this liquid. If the red and the blue matter contained in the mulberry be considered as distinct principles, the author proposes to call the former Pyramus, the latter Thisbe.

The colouring matter of the corn-poppy is scarcely altered by carbonate of soda. Caustic potash makes it green, and caustic ammonia produces no effect. Muriatic acid renders the infusion of the poppy petals florid red, which is rendered dark red by carbonate of lime. These and other experiments induce Mr. Smithson to regard the colour of the corn-poppy as analogous to the red principle of the mulberry.

The pigment called sap green is the inspissated juice of the buckthorn berries. It is rendered yellow by carbonate of soda and caustic potash. Its solution is reddened by acids, and the green is restored by chalk. To this substance, and to the common green matter of vegetables, the author assigns the name Chloris.

The colour of some green insects is not altered either by muriatic acid or carbonate of soda, and therefore appears to be a peculiar principle differing from that of vegetables.

Account of Experiments made on the Strength of Materials. By George Rennie, *jun. Esq. In a Letter to* Thomas Young, *M.D. For. Sec. R.S.* Read February 12, 1818. [*Phil. Trans.* 1818, *p.* 118.]

After taking a cursory view of the labours of others in this department of mechanical inquiry, Mr. Rennie proceeds to give an account of the apparatus which he employed, and of the result of his own experiments. Of the resistances opposed to the simple strains which may disturb the quiescent state of a body, the principal are: the repulsive force, whereby it resists compression; and the force of cohesion, whereby it resists extension. On the former, with few exceptions, there is scarcely anything on record. Lagrange, in his Memoir on the Force of Springs, published in 1760, represents the moment of elasticity by a constant quantity, without indicating the relation of this value to the size of the spring: but in the Memoir of 1770, on the Forms of Columns, when he considers a body whose dimensions and thickness are variable, he makes the moment of elasticity proportional to the fourth power of the radius:—but all these calculations, says Mr. Rennie, are inapplicable to columns under common circumstances. The results of experiments are also extremely discordant; for it is deduced from those of Reynolds, that the power required to crush a cubic quarter of an inch of cast iron is 200 tons, whereas in the author's experiments upon cubes of the same size, the amount never exceeded five tons; and although Mr. Reynolds probably employed metal cast at the furnace of Maidley Wood, which is very strong, yet this circumstance can have been but of little importance compared with the great disproportion of results.

Mr. Rennie employed four kinds of iron: the first taken from the centre of a large block, similar in appearance to what is usually called gun metal; the second from a small casting, close-grained, and of a dull gray colour; the third, horizontally cast iron, in bars three eighths of an inch square and eight inches long; the fourth, similar bars cast vertically. It appears from the annexed tables that the vertical castings are stronger than those taken from the block.

Some miscellaneous experiments relating to the different kinds of wood and stone are also added to those on the metals. They show that little dependence can be placed on the specific gravity of the stone; neither is hardness to be regarded as a characteristic of strength. In the rupture of amorphous stones, Mr. Rennie remarks, that pyramids are formed, having for their base the upper side of the cube next the lever, the action of which displaces the sides of the cubes precisely as if a wedge had operated between them.

adopted, "that membranes upon which no glandular structure can be discovered are capable of secreting mucus," is confirmed.

From the annexed chemical examination of the Javanese swallow's nest, it appears to possess a close analogy to albumen; differing from the ordinary properties of that principle in being easily soluble in liquid ammonia, and in the solution of its subcarbonate, and in affording a relatively smaller proportion of ammoniacal products when submitted to destructive distillation.

This paper is accompanied by a drawing, exhibiting magnified representations of the gastric glands in the blackbird, and in the common and Java swallow; thus rendering the differences of structure, so as to preclude the necessity of any extensive details.

Observations on the Hirudo complanata, *and* Hirudo stagnalis, *now formed into a distinct Genus under the name*, Glossopora. *By Dr.* Johnson, *of Bristol. Communicated by Sir* Everard Home, *Bart. V.P.R.S.* Read June 26, 1817. [*Phil. Trans.* 1817, *p.* 339.]

The animals named in the title of this paper differ so considerably from the Leech, as to induce the author to remove them from the genus Hirudo, and to form them into a distinct one under the term Glossopora, a term derived from a prominent feature of the animal, namely, its projectile tubular tongue.

They resemble the leech, in the body being furnished with a series of rings, in locomotion being effected by the alternate motion of the head and tail, and in the division of one general stomach into several lateral cells or partitions. They differ from the leech in the mouth being furnished with a projectile tubular tongue; in the flat pyriform shape of the body; and in having an abdominal pouch or cavity for the reception of their young. After enumerating the character of the genus, Dr. Johnson expresses his opinion that the *Hirudo circulans*, *Hirudo crenata*, *Hirudo hyalina*, and *Hirudo tessulata*, will be found to belong to it; and the *Hirudo sexoculata*, described by Bergmann in the Stockholm Transactions, seems to be the same animal. Its tongue is cartilaginous, flexible, and about one eighth of an inch long. The author describes the *Glossopora tuberculata* and the *Glossopora punctata*. The notion that they are capable of reproduction when cut or divided, he considers without foundation. Their food consists chiefly of water Helices; into the shell of which they easily penetrate in consequence of their tapering head, and from the flexibility of the tongue they are enabled to follow their victim to the innermost recess of its habitation. The ova are received into the abdominal pouch of the parent, where they remain till fully evolved, and they are unproductive if moved from this situation.

An annexed drawing illustrates the anatomy and habits of these animals.

Observations on the Gastric Glands of the human Stomach, and the Contraction which takes place in that Viscus. By Sir Everard Home, *Bart. V.P.R.S.* Read June 26, 1817. [*Phil. Trans.* 1817, *p.* 347.]

This paper contains an account of the internal membrane of the human stomach, in reference to magnified views of the different structures composing its surface, executed by Mr. Bauer.

The œsophageal glands have the appearance of infundibular cells. The structure upon the upper arch of the stomach is made up of cells, of the form of a honeycomb; and this structure extends, though less visibly, over the whole surface of the cardiac portion. In the pyloric portion the cells have the same appearance; but there are small clusters, the sides of which rise above the surface, giving the appearance of foliated membranes.

Having formerly shown that the gastric glands are largest and most numerous in the animals that inhabit the least fertile regions of the earth, and *vice versâ*, the author remarks the greater necessity for the same arrangements in man, whose gastric glands are so small as to require microscopic aid to prove that they appertain to the same series of structures as those of the ostrich, which may be minutely examined by the unaided eye.

Sir Everard alludes to his former discovery of the occasional division of the stomach into two portions by a muscular contraction, which he is now able further to elucidate by a case in which this contraction had become permanent, and which probably caused the death of the woman in whom it occurred. The importance of this fact in studying the physiology of the stomach, is the only apology, says the author, which I shall make for having pressed it so much on the attention of the Society: its use in the pathology of that viscus, though perhaps of still more importance to the cause of suffering humanity, this is not the proper place to consider.

A drawing of the contracted stomach also is annexed to this paper.

On the Parallax of the fixed Stars. By John Pond, *Esq. Astronomer Royal.* Read June 26, 1817. [*Phil. Trans.* 1817, *p.* 353.]

The object of this paper is to communicate a series of observations made with a new instrument for the purpose of investigating the question of parallax. Though a much longer period of time will be necessary to elucidate this subject in a perfectly satisfactory manner, yet, from the observations already made, it seems highly probable that the parallax of α Cygni is too small a quantity to have had any share in producing either the discordances remarked by Dr. Brinkley, or those in the Greenwich observations already communicated to the Society.

The method consists in continually observing the meridional difference in polar distance of α Cygni and β Aurigæ, (which pass through the field of the same telescope,) by means of a micrometer adapted to this purpose.

A drawing of the instrument accompanies the observations.

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becomes tubular, afterwards vascular, and then takes the form of healthy granulations.

On the Laws of Polarization and Double Refraction in regularly Crystallized Bodies. By David Brewster, LL.D. F.R.S. Lond. and Edin. In a Letter to the Right Hon. Sir Joseph Banks, Bart. G.C.B. P.R.S. Read January 15, 1818. [*Phil. Trans.* 1818, *p.* 199.]

In the different inquiries which the author has already laid before this Society, his attention was often directed to the phenomena of regular crystals; but he only lately succeeded in reducing under a general principle all those complex appearances which result from the combined action of more than one axis of double refraction. In this paper Dr. Brewster gives a general view of the present state of our knowledge respecting the double refraction and polarization of light, and afterwards traces the steps which led him to the discovery of the general law. He began his researches by the examination of 165 crystals, in 145 of which he discovered the property of double refraction. In 80 he was able to ascertain whether they had one or more axes; and by examining the tints which they exhibited at various angular distances from the axes, whence the forces emanate, he has been led to a general principle, which embraces all the phenomena and extends to the most complex as well as to the most simple development of the polarizing forces. This general principle, says Dr. Brewster, is in no respect an empyrical expression of the facts which it represents, nor is it supported by any empyrical data. Founded on the principles of mechanics, it is a law rigorously physical, by which we are enabled to calculate all the tints of the coloured rings, and all the phenomena of double refraction, with as much accuracy as we can compute the motions of the heavenly bodies.

The faculty of depolarization, explained by the author in a former paper, has been considered as sufficient indication of two separate images; and upon this principle it has been stated that all crystals are doubly refractive whose primitive form is neither the cube nor the regular octohedron: but this is incorrect; for some of these crystals possess a doubly refracting structure in a high degree. Admitting the statement, however, it could not have been used as a rule for determining whether a crystal refracts doubly or singly; for it is more difficult to detect the primitive form than to examine the optical properties. Tungstate of lime, for instance, would have been reckoned a crystal without double refraction, when Haüy believed its primitive form to be the cube, although it is highly doubly refractive.

In examining the nature and properties of the coloured rings produced by certain crystals, the author found that the squares of the diameters of the rings were, in every case, proportional to the numbers which represent the corresponding tints in Newton's table.

When a plate of beryl was combined with a plate of calcareous spar, the system of rings was the same as would have been produced by two plates of beryl, one of which was the plate employed, and

the other a plate which gave rings of the same size as the plate of calcareous spar. But when we combine a system of rings produced by a crystal of zircon, with the system produced by calcareous spar, a different effect is produced; and the system, instead of being diminished, is increased, and is equal to that which would have been produced by a thin plate of calcareous spar, whose thickness is equal to the difference of the thicknesses of the plate of calcareous spar employed, and the plate of calcareous spar that would give rings of the same size as those given by the zircon alone. In the section "on crystals with two or more axes of polarization," Dr. Brewster observes that, although M. Biot considered mica as the only mineral possessing the compound structure indicating two axes, he had found the same structure in topaz, nitre, tartrate of potash and soda, sulphate of potash, acetate of lead, and mother-of-pearl, as early as 1813; and he points out the means of deducing the number of axes in crystals from their primitive forms. Dr. Brewster expresses the general law of the tints for crystals with one or more axes in the following manner. *The tint produced at any point of the sphere by the joint action of two axes is equal to the diagonal of a parallelogram whose sides represent the tints, and whose angle is double the angle formed by the directions in which the forces are exerted.*

The fourth and fifth sections of this paper relate to the resolution and combination of polarizing forces, and the reduction of all crystals to those with two or more axes; and to the polarizing structure of crystals that have the cube, the regular octohedron, and the rhomboidal dodecahedron for their primitive form. The sixth and concluding section describes the artificial imitation of all the classes of doubly refracting crystals, by means of plates of glass; in which the author demonstrates that the polarizing structure depends entirely upon the external form of the plate, and on the mode of aggregation of its particles. When its form is circular, it has only one axis of polarization, which is attractive if the density diminishes towards the centre, and repulsive if it increases towards the centre; but when its form is rectangular or elliptical, it then has two axes of polarization, the strongest of which appears to be attractive, and the weakest repulsive. The elementary spheroid of crystals with double axes may be supposed, says the author, to be formed by elliptical plates bent into spheroidal strata; and the spheroid itself may be constructed by spheroidal strata of glass, it then exhibiting all the complicated phenomena produced by the simultaneous actions of two unequal axes.

On the Parallax of certain fixed Stars. By the Rev. John Brinkley, *D.D. F.R.S. and Andrews Professor of Astronomy in the University of Dublin.* Read March 5, 1818. [*Phil. Trans.* 1818, *p.* 275.]

Since the author's former observations on the parallax of *α* Lyræ, published by the Royal Society in a Letter to Dr. Maskelyne, he (the author) has met with apparent motions in several of the fixed stars,

which he could only explain by referring them to parallax. Among these stars, α Aquilæ exhibited the greatest change of place.

In consequence of the Astronomer Royal having doubted the correctness of the author's conclusions upon this point, he has anxiously engaged in observations relating to it during the last sixteen months; and although the results in respect to α Lyræ and to Arcturus have not been very uniform, the recent observations on α Cygni are consistent with the former ones, and exhibit the same discordance between the summer and winter observations as before. In regard to α Aquilæ also, the observations detailed in the present paper are remarkably coincident with those formerly detailed; and the author thinks that it is to this star we must look for the final decision of the question concerning parallax.

Referring to Mr. Pond's observations, Dr. Brinkley is led to entertain doubts of the fitness of an instrument similar to the Greenwich mural circle for so delicate an inquiry, founded upon remarks detailed in the paper respecting the elements used in computing the index error, and which are independent of the uncertainties to which the observation itself is also subject. It is, however, from the uncertainty of the elements used in the reductions, and not from any errors of the observations, or from any defect in the construction of the instrument alluded to, that Dr. Brinkley is induced to consider the observations hitherto made at Greenwich as not affording conclusive results as to the existence or non-existence of parallax. In the present state of astronony, however, it will be allowed that the relative fitness of instruments for ascertaining with precision the smaller motions, whether real or apparent, of the fixed stars, is an object of importance.

On the Urinary Organs and Secretions of some of the Amphibia. By John Davy, *M.D. F.R.S. Communicated by the Society for the Improvement of Animal Chemistry.* Read April 2, 1818. [*Phil. Trans.* 1818, *p.* 303.]

In several species of serpents which were examined by Dr. Davy, the kidneys were nearly as large as the liver, long, narrow and lobulated, and without a pelvis. Each lobule sends a small duct to the ureter, which terminates in a papilla situated in the cloaca, between the mouths of the oviducts, and having its point directed towards a receptacle for the urine, which, though a continuation of the intestine, may be considered as distinct from the rectum and cloaca, with which it communicates only by sphincter orifices.

The urinary ducts often contain a white matter, visible through their coats, which gradually accumulates in the receptacle till it forms a mass which, when of so large a size as to distend the part, is usually expelled by an extraordinary effort of the animal, most commonly in the act of devouring its food. The urine, at first soft, gradually hardens by exposure, and then looks like chalk; it consists of nearly pure uric acid.

The author next relates his experiments and observations upon the urine and urinary organs of lizards. He examined four species, —the gecko, iguana, the kobbera-guion, (described by Knox,) and the alligator. The kidneys vary in size; each ureter has a papilla situated in the receptacle; in other respects the structure resembles that of snakes. The secretion is also nearly similar; that of the alligator contains, besides uric acid, carbonate and phosphate of lime; in one case it smelt strongly like musk.

In two species of the testudo, Dr. Davy found the kidneys lobulated like those of the preceding animals. In the bladder both of the turtle and tortoise he found flakes of uric acid in a transparent liquid, containing mucus and common salt, but no urea.

On a Mal-conformation of the Uterine System in Women; and on some Physiological Conclusions to be derived from it. In a Letter to Sir Everard Home, *Bart. V.P.R.S. from* A. B. Granville, *M.D. F.R.S. F.L.S. Physician in ordinary to H. R. H. the Duke of Clarence; Member of the Royal College of Physicians, and Physician-Accoucheur to the Westminster General Dispensary.* Read April 16, 1818. [*Phil. Trans.* 1818, *p.* 308.]

The uterus described in this paper had acquired its full development upon the right side only. The left side exhibited a straight line, about half an inch distant from its centre. Upon this side also all the appendages of the uterus were deficient, though their rudiments might be traced. This woman was the mother of eleven children of both sexes, and had been delivered of twins, male and female, a few days before her death, which was occasioned by diseased heart and aneurism of the aorta.

Dr. Granville remarks that this is the first case upon record which disproves the opinion that the different sides of the uterus are concerned in the production of the two sexes. It also shows that twins of both sexes may be derived from one ovarium.

This paper concludes with some remarks upon supposed cases of superfœtation.

New Experiments on some of the Combinations of Phosphorus. By Sir H. Davy, *LL.D. F.R.S. V.P.R.I.* Read April 9, 1818. [*Phil. Trans.* 1818, *p.* 316.]

Since the author's former communication upon the above subject to the Royal Society, various researches have been brought forward, differing in their results from his own as well as from each other. Sir Humphry concluded that the phosphoric acid contained about three fifths its weight of oxygen, or twice that contained in the phosphorous acid. Berzelius considers the phosphoric acid as composed of 100 phosphorus + 128·17 oxygen; and Dulong, of 100 phosphorus + 124·5 oxygen: and both these chemists consider the

oxygen in the phosphorous acid to be to that in the phosphoric as 3 to 5.

After showing that the only possible source of error in his former experiments was the smallness of the quantity of the phosphorus burned, Sir Humphry describes various modes of effectually carrying on the combustion upon a larger scale, and gives the preference to that in which the vapour of phosphorus, passing from the orifice of a small tube, is made to burn in a retort filled with pure oxygen. The mean result of several experiments carefully conducted upon this plan, gave the composition of phosphoric acid at 100 phosphorus + 134·5 oxygen.

The author having shown the insufficiency of Dulong's method for ascertaining the composition of phosphoric acid and of the chlorides of phosphorus, proceeds to detail his researches upon the latter compounds, and upon the constitution of phosphorous acid. The result of several experiments indicated the composition of perchloride of phosphorus to be 1·00 of phosphorus + 6 of chlorine; and showed that phosphorous acid contained half the quantity of oxygen existing in the phosphoric acid, and the liquid chloride half the quantity of chlorine contained in the solid perchloride. These experiments sufficiently agree with each other to afford the means of determining the equivalent number of phosphorus. Thus, if phosphoric acid be supposed to consist of two proportions of oxygen and one of phosphorus, the number representing the proportion in which phosphorus combines will be 22·3. If the absorption of chlorine in forming phosphorane be made the datum, the number will be 22·2. If the quantity of horn silver formed from the liquid chloride be assumed as the datum, the number will then be 23·5. The mean of all is 22·6, or the double 45·2, from which, if we take away the decimal, we obtain 45. The author's experiments upon phosphate of potash also agree with this number.

The next subject discussed in this paper is the hypophosphorous acid of M. Dulong. Although Sir Humphry has satisfied himself of the existence of this acid, he is not disposed to regard the methods of analysis adopted by its discoverer as satisfactory. When hypophosphite of baryta is decomposed by heat, it is converted into phosphate of baryta and hydrophosphoric gas; and knowing the quantity of acid in the former, and of phosphorus in the latter, it is easy in this way to learn the composition of the hydrophosphorous acid. The results of Sir Humphry's experiments, however, lead to the conclusions adopted by Dulong; namely, that the quantity of oxygen in the hydrophosphorous acid is half that which is contained in the phosphorous acid. M. Dulong has suggested that the acid described by the author as a mixture of phosphorous and phosphoric acids, is a peculiar chemical compound, and proposes to call it phosphatic acid; but as it has no crystalline form, nor any marked characters; as it is not of uniform composition; and as phosphorous and phosphoric acids mixed, produce a substance of the same kind,—Sir Hum-

phry does not admit of this conclusion. The author has adopted throughout the calculations in this paper the supposition that the hydrogen in water is as 2 to 15 to the oxygen; and consequently, he says, has taken the number 15 to represent the latter element. If the hypophosphorous acid be regarded as a simple compound of oxygen and phosphorus, it will consist of 45 phosphorus + 15 oxygen; phosphorous acid of 45 phosphorus + 30 oxygen; phosphoric acid of 45 phosphorus + 60 oxygen.

Sir Humphry concludes this paper with some incidental observations relating to the compounds of phosphorus.

New Experimental Researches on some of the leading Doctrines of Caloric; particularly on the Relation between the Elasticity, Temperature, and latent Heat of different Vapours; and on Thermometric Admeasurement and Capacity. By Andrew Ure, *M.D. Communicated by* W. H. Wollaston, *M.D. F.R.S.* Read April 30, 1818. [*Phil. Trans.* 1818, *p.* 338.]

This paper is divided into three sections. In the first the author, after taking an historical view of the different experiments undertaken by Robinson, Watt, Dalton, Biot, and some others, relating to the elastic force of vapours arising from different bodies at different temperatures, and after pointing out the sources of error and imperfection to which they are liable, proceeds to describe the apparatus which he employed, which is further illustrated by an annexed drawing. The space which contains the vapour for experiment is about half an inch of a barometer tube, against which the oblong bulb of a delicate thermometer rests so as to indicate the true temperature. The contrivance is such, that though the liquid and incumbent vapour are restricted to the summit of the tube, its progressive range of elasticity may be measured from 0° to 200° above the boiling point of water, or from an elasticity of 0·07 inch to that capable of sustaining 36 feet of mercury, without heating the mercurial column itself. In this section of the paper are several tables of results, showing the elastic force of the vapour of water in inches of mercury, at temperatures between 24° and 312°; and also that of alcohol, ether, oil of turpentine, and naphtha. The second section of Dr. Ure's paper relates to thermometric admeasurement, and to the doctrine of capacity. He does not consider the thermometer liable to the uncertainties which are supposed to belong to it by Mr. Dalton, but that it is an equable measure of heat, in consequence of its possessing an increasing rate of expansion, and which is compensated for by a quantity of the quicksilver getting out of the bulb into the tube, and consequently out of the action of the heat, the bulb being the only part heated in all ordinary cases.

In the third section, relating to the latent heat of different vapours, Dr. Ure details experiments made to ascertain the caloric existing in different vapours, and the temperatures at which they respectively acquire the same elastic force.

The apparatus employed consisted of a small glass retort only, the globular receiver being surrounded by a certain quantity of water of known temperature. Two hundred grains of the liquid, whose vapour was to be examined, were rapidly distilled from this retort into the globe, and the rise of temperature in the surrounding water became the measure of the latent heat. A table follows, exhibiting the experimental results on the latent heat of several vapours: whence it appears that 967° is the latent heat of steam, 442° that of alcohol; of ether, 302·3°; of oil of turpentine and of petroleum, 177·8°; of nitric acid, 531·9°; of liquid ammonia, 837·2°; and of vinegar, 875°.

The paper concludes with a proposal for employing the vapour of alcohol in certain cases, for the purpose of propelling machinery.

Observations on the Heights of Mountains in the North of England. By Thomas Greatorex, *Esq. F.L.S. In a Letter to* Thomas Young, *M.D. For. Sec. R.S.* Read May 7, 1818. [*Phil. Trans.* 1818, *p.* 395.]

Wishing to measure Skiddaw geometrically, the author employed a staff about 28 feet long. Its graduation commenced at 0°, placed about 3 feet above its lower end, from which to the top was exactly 25 feet. A stationary barometer was next placed 10 yards above the lake, and its variation and that of a thermometer were noted every half hour. Another barometer and thermometer were then set upon the summit of the mountain, and their respective heights accurately observed. A telescope, with cross wires, was then carefully levelled, and the wires made to intersect the highest point of the mountain. It was then pointed in the direction of the most convenient descent, and the staff carried down the hill till its top exactly coincided with the cross wires, the level of the telescope being carefully preserved. The perpendicularity of the staff was ascertained by plumb lines; and as it was seldom more than 40 feet from the telescope, no allowance was necessary for the earth's curvature. The most exact mode of managing the pole, says the author, was to stop my assistant when I observed its top to be about an inch above the cross wires, and then it was pressed gradually into the earth till an exact coincidence was obtained. The telescope was then carried down to the pole, levelled and placed in exact correspondence with zero. The pole was again carried to a new station, and this mode continued for fifty yards of descent. The barometer was then again set up and examined, and the process continued to the foot of the mountain. The height of Skiddaw, by levelling, was 1012 yards 3¼ inches.

Annexed to this paper are the results of several barometrical observations made on the summit of Skiddaw, and continued at different distances of fifty yards each down to the foot of the mountain.

On the Different Methods of Constructing a Catalogue of Fixed Stars. By J. Pond, *Esq. F.R.S. Astronomer Royal.* Read May 21, 1818. [*Phil. Trans.* 1818, *p.* 405.]

The method hitherto adopted in the Royal Observatory for constructing a catalogue of stars, either in declination or right ascension, has been to take some one star as a point of departure, and thus to determine the position of the rest by direct comparison. The declinations were determined by direct comparison with γ Draconis, and α Aquilæ was chosen as the common term of comparison in right ascension. In observations with the transit instrument, this mode of proceeding is highly objectionable; for every result is subject to a double error,—that committed in the observation of α Aquilæ, and that in the observation of the star itself. Besides which, if the observation of α Aquilæ be omitted, then the other observations become useless. Hence, although extreme accuracy was ultimately thus obtained by the late Astronomer Royal, the method was tedious and objectionable.

The method which Mr. Pond proposes to substitute, and which he describes in this paper, has the advantage of affording, in a single year, a catalogue equally accurate with one formerly obtained in three, and equally applicable to the mural circle and transit instrument. No particular star is in either case assumed as a point of departure in preference to the rest. On the contrary, every star is in its turn assumed as a point of reference to the others. It is thus endeavoured, in the first instance, to establish their relative distances from each other, or from the equator or meridian, leaving the choice and determination of some common point of departure as a subject for future consideration. The principles of proceeding applicable to both instruments are then detailed at length, and the striking coincidence of the author's catalogue, and that of the late Dr. Maskelyne, adverted to.

In respect to the accuracy of the results afforded by the new transit instrument, Mr. Pond thinks that 120 observations enable him to define the place of a fixed star to one tenth of a second of a degree.

A Description of the Teeth of the Delphinus Gangeticus. *By Sir* Everard Home, *Bart. V.P.R.S.* Read June 4, 1818. [*Phil. Trans.* 1818, *p.* 417.]

In the 7th volume of the Asiatic Researches, published in 1781, Dr. Roxburgh describes the *Delphinus Gangeticus*, but gives a very imperfect account of its teeth; nor is any detailed account of them given in any other work. As the jaws and teeth of this species of Delphinus form its most remarkable character, Sir Everard thinks the subject of sufficient interest to the comparative anatomist and geologist, to be laid before this Society.

These teeth, as in the whole tribe, generally have the rudiments in the gums, from which the teeth grow in both directions; upwards

through the gum in the form of the point of a flattened cone, which is coated with enamel, and downwards towards the jaw, increasing in breadth, but not in thickness, till it is imbedded in the substance of the jaw itself. The lower portion has no enamel; the number of teeth is, as described by Dr. Roxburgh, 120.

Description of an Acid Principle prepared from the Lithic or Uric Acid. By William Prout, *M.D. Communicated by* W. H. Wollaston, *M.D. F.R.S.* Read June 11, 1818. [*Phil. Trans.* 1818, *p.* 420.]

The object of this paper is to show that the purple substance obtained by heating a mixture of the lithic and nitric acids, is a compound of ammonia with a peculiar acid principle, which the author proposes to call Purpuric Acid, a term suggested by its peculiar tendency to form red or purple compounds.

The purpuric acid is obtained by digesting pure lithic acid in dilute nitric acid, neutralizing the excess of the latter by ammonia, and evaporating till granular crystals, consisting of purpurate of ammonia, separate. The ammonia is removed by sulphuric or muriatic acid, and the purpuric acid thus obtained in a free state.

The author next points out the characters of this acid. It is very sparingly soluble in water, and insoluble in alcohol and ether. In the mineral acids, and in the alkalies, it readily dissolves. It is insoluble in dilute sulphuric, muriatic, phosphoric, oxalic, citric, and tartaric acids. When heated it neither melts nor sublimes, but becomes purple, from the production of ammonia, and then burns gradually without any particular odour. It unites with the metallic oxides; and when aided by heat, expels carbonic acid from the alkaline carbonates. It does not unite with any other acid. Upon these characters the author thinks that its properties, as an acid, are sufficiently established.

Dr. Prout then proceeds to describe its compounds with different bases, which, with few exceptions, are of a purple or reddish colour: he thinks that some of them might be used as pigments, or employed in the art of dyeing.

Astronomical Observations and Experiments, selected for the purpose of ascertaining the relative Distances of Clusters of Stars, and of investigating how far the Power of our Telescopes may be expected to reach into Space, when directed to ambiguous Celestial Objects. By Sir William Herschel, *Knt. Guelp. LL.D. F.R.S.* Read June 11, 1818. [*Phil. Trans.* 1818, *p.* 429.]

Having shown in a former paper that by an equalization of the light of stars of different brightness, their relative distances from the observer in the direction of the line in which they are seen may be ascertained, and having deduced from this equalization a method of turning the space penetrating power of a telescope into a gradually

increasing series of gauging powers, by which the profundity in space of every object consisting of stars can be ascertained, as far as the light of the instrument will reach, Sir William Herschel proceeds to make use of some of his numerous observations made upon those occasions, to show how the distances of globular and other clusters of stars may be obtained, and has represented their situations in space by a figure, in which their distances are made proportional to the diameter of a globular space, sufficiently large to contain all the stars that are visible to the eye of an observer in the clearest nights.

The author then details a series of observations of clusters of stars, from which the order of their profundity in space is determined, and describes the manner in which he represents the profundity of celestial objects in space by diagrams; and in the concluding section of his paper, considers the extent of the power of telescopes to reach into space when they are directed to ambiguous celestial objects.

On the Structure of the Poisonous Fangs of Serpents. By Thomas Smith, *Esq. F.R.S.* Read June 4, 1818. [*Phil. Trans.* 1818, *p.* 471.]

The object of this paper is to explain the existence of a slit in the fangs of serpents, extending from the foramen at the base to the aperture near the point, and to show that this slit is caused by the manner in which the tube through which the poison flows is formed. After describing the growth of the teeth of poisonous serpents, the author observes, that in those which are not venomous, there are no traces of any furrow or depression.

A drawing, illustrating the author's description, is annexed to this paper.

On the Parallax of α Aquilæ. By John Pond, *F.R.S. Astronomer Royal.* Read April 16, 1818. [*Phil. Trans.* 1818, *p.* 477.]

The telescope erected for this investigation resembles in its construction that which was formerly used for the observations of α Cygni. It has an achromatic object-glass of 10 feet focal length, and 4 inches diameter.

The Astronomer Royal had first selected β Canis Minoris as a proper star to be compared with α Aquilæ; but finding, upon trial, that it could rarely be seen in the day-time, he was induced to substitute λ Pegasi. Not being quite satisfied of the stability of the instrument, the author has only computed those observations in which each star was observed in the same day, and in the short interval of three hours; so that it was not likely any sensible change in the telescope should have taken place. The result of fifty-four observations between the 25th of July and the 29th of December 1817, afforded no appearance of parallax; indeed the author considering it as a hopeless task to establish its existence by observations on a star so far from the zenith, was about to abandon the subject, when his at-

tention was again called to it by Dr. Brinkley's late communication. By reference to an annexed table, it appears that the greatest error in a series of ten observations, made with the transit, could not have been more than 0·″03, and consequently it is not probable that the error in fifty observations should have exceeded half that quantity. Taking, however, every circumstance into consideration, it is possible that the whole parallax of *α* Aquilæ may have amounted to half a second, which is about a tenth part of that assigned to this star by Dr. Brinkley. The author, however, proposes to continue the investigation; and when his observations shall have been sufficiently multiplied, promises to communicate the result to the Society.

On the Parallax of the Fixed Stars in Right Ascension. By John Pond, *F.R.S. Astronomer Royal.* Read May 28, 1818. [*Phil. Trans.* 1818, *p.* 481.]

This paper is intended as an appendix to a former one on the same subject. The author extends his investigation to a few more of the principal fixed stars. He divides the results of any one star into two parts; first, alternately or accidentally, and also according to the law of parallax; and as no greater difference is observable in the latter than in the former case, it is demonstrable that parallax has had no sensible effect on the observation. He next inquires what may be the magnitude of the parallax that might be concealed by the accidental error of observation. Without entering into a rigorous computation on the laws of probability, he conceives that it may be inferred by inspection, that it is almost impossible that the longer axis of the ellipse, described by the brightest fixed star, can exceed 0″·6, and it is very improbable that it should amount to half as much; and as this quantity can never derange the mean place of a star 0″·1 in declination, it is evident that all attempts to determine the parallax by a meridian instrument of any description must be utterly hopeless.

An Abstract of the Results deduced from the Measurement of an Arc on the Meridian, extending from Latitude 8° 9′ 38″·4, *to Latitude* 18° 3′ 23″·6 *N.*, *being an Amplitude of* 9° 53′ 45″·2. *By Lieut. Colonel* William Lambton, *F.R.S.* 33*rd Regiment of Foot.* Read May 21, 1818. [*Phil. Trans.* 1818, *p.* 486.]

The author, at the commencement of this paper, refers to the 12th volume of the Asiatic Researches, in which there are detailed accounts of two complete sections of an arc on the meridian, measured by him in prosecuting the Trigonometrical Survey of the Peninsula of India. The first is comprehended between the parallels of Punnæ, a station near Cape Comorin, in latitude 8° 9′ 38″·39, and Patchipolliam in Coimbetoor, in latitude 10° 59′ 48″·93. The second is comprehended between the parallels of Patchipolliam and Namthabad, a station near Gooty in the ceded districts, in latitude 15° 6′ 0″·21,

Since those measurements, the author has obtained another section, extending from Namthabad to Daumergidda, in the Nizam's dominions, which being in latitude 18° 3′ 23″·6, gives a total arc of 9° 53′ 45″·14 in amplitude.

From the first of these sections, Colonel Lambton finds the length of the degree due to latitude 9° 34′ 44″ (the middle point of that arc), equal to 60472·83 fathoms. The second section, whose middle point is in latitude 13° 2′ 55″, gives the mean degree equal to 60487·56 fathoms; and the last section gives the degree equal to 60512·78 fathoms due to the latitude of 16° 34′ 42″, the middle point of that section.

The author proceeds to compare each of these degrees, first with the French measure, then with the English, and lastly, with the Swedish measure, and thence obtains a general mean for the compression at the poles. The first mean of these three degrees, used with the French degree, gives the compression $\frac{1}{308.18}$; the second mean of the same three degrees, used with the English degree, gives $\frac{1}{313.88}$; and the third mean of these three degrees, used with the Swedish degree, gives $\frac{1}{307.18}$; so that the mean of these three means will give the compression $\frac{1}{309.00}$, or $\frac{1}{310}$ nearly of the polar axis.

The number of base lines in this extensive arc are five, all measured with the chain extended in coffers, with elevating screws, &c.

The author, after giving a variety of data, proceeds to investigate the formulæ which he has employed in his calculations, and concludes with a table of the lengths of different degrees for every third degree from the equator to the pole.

The Croonian Lecture. On the Conversion of Pus into Granulations or New Flesh. By Sir Everard Home, *Bart. V.P.R.S.* Read November 5, 1818. [*Phil. Trans.* 1819, *p.* 1.]

The changes which pus undergoes in the formation of new flesh are so analogous to those which take place in the blood, and which were discussed in the author's Croonian lecture of last year, that he is induced to consider the two fluids as possessed of the same properties, the colour of the globules being the principal characteristic distinction between them.

That pus is a transparent fluid, in which globules are subsequently formed, was proved by the author in 1788; and in July 1817 Mr. Bauer observed the same property in the serum of the blood: he saw globules forming in that fluid while he was examining it in the field of the microscope. Human blood, sheep's blood, and calves' blood, presented to Sir Everard similar results; the serum of which, when warm and fresh, was observed, in a space covering $\frac{1}{160000}$th part of an inch, to produce from six to twelve globules in a few minutes, two only being observed in the first instance.

The author, after detailing further experiments on the formation of globules in the serum of the blood, proceeds to examine the changes which happen in pus upon the surface of a sore, having previously

described the appearance of the surface immediately under the newly secreted pus. It was made up of eminences and hollows; the former consisting of clusters of tortuous blood-vessels, the latter filled with pus. After a few minutes' exposure, the following changes were observed: a transparent pellicle covered the surface, under which globules of air made their appearance in different places; then horizontal anastomozing canals, filled with red blood, were seen to form; and red spots, which were the termination of perpendicular canals, were observed under the pellicle. Drawings illustrating these appearances accompany the paper.

After detailing further proofs and illustrations of the above phenomena, presented by the surfaces of sores covered with pus, Sir Everard observes, that the carbonic acid originally contained in the tubes is very readily displaced by the blood, in consequence of its disposition to absorb that particular gas which forms so large a proportion of its component parts. He thinks that the extrication of carbonic acid is the original cause of the tubularity of pus; and that the tubes are then filled with red blood, and thus connected with the circulation. The succeeding changes are illustrated by Mr. Bauer's drawing, which the author laid before the Society last year.

On the Laws which regulate the Absorption of Polarized Light by Doubly Refracting Crystals. By David Brewster, *LL.D. F.R.S. Lond. and Edinb. In a Letter addressed to the Right Hon. Sir* Joseph Banks, *Bart. G.C.B. P.R.S.* Read November 12, 1818. [*Phil. Trans.* 1819, *p.* 11.]

In examining the polarizing structure of acetate of copper, the author's attention was drawn to certain changes of colour exhibited by its crystal, when exposed in different positions to polarized light; and as these were independent of the thickness of the plate, and of any analysis of the transmitted pencil, he was induced to regard them as a new affection of light, ascribable to the absorption of the homogeneous tints forming the compound colour of the crystal. Dr. Brewster, therefore, collected a variety of coloured crystals, with a view to examine the phenomena which they presented, when cut at different angles with the axis, and when exposed in different positions to polarized light. The details of this examination are next given; and as the property of transparent bodies, by which they detain and assimilate to their own substance a portion of the rays which penetrate them while the rest are freely transmitted, is related to the axes of double refraction, the author first describes the phenomena presented by crystals of one axis, and then explains the modifications which they undergo when the number of axes is increased.

It appears from these investigations that the colouring particles of crystals, instead of being indiscriminately dispersed throughout their mass, have an arrangement related to the ordinary and extraordinary forces which they exert upon light. In some cases, the extraordinary

medium appeared to be tinged with the same kind and number of colouring particles as the ordinary medium; but in other cases, in the same mineral, the extraordinary medium was either tinged with a different number of particles of the same colour, or with a colouring matter entirely different from that of the ordinary medium. In some specimens of topaz the colouring matter of the one medium was more easily discharged by heat than that of the other, one of the pencils being yellow and the other pink: hence it is a mistake to suppose that in converting yellow topazes into pink by heat, the former colour is changed into the latter; the fact being, that the yellow is discharged by heat, thus leaving the pink unimpaired. Hence it may be ascertained beforehand whether a topaz will receive a pink colour by heat; for if that colour exist in one of its images, seen by exposing it to a polarized ray, we may predict the success of the experiment.

In two specimens of emerald it was found that the colouring matter which tinged the ordinary medium in the one, tinged the extraordinary medium in the other, and *vice versâ*.

Observations sur la Décomposition de l'Amidon à la Température Atmosphérique par l'Action de l'Air et de l'Eau. Par Théodore de Saussure, *Professeur de Minéralogie dans l'Académie de Genève, Correspondant de l'Institut Royal de France, &c. Communicated by* Alexander Marcet, *M.D. F.R.S.* Read December 17, 1818. [*Phil. Trans.* 1819, *p.* 29.]

After some general observations on the changes which starch undergoes during the process of germination, and also when acted on by dilute sulphuric acid, in the manner contrived by M. Kirchoff, the author proceeds to show that starch alone, boiled in water and left to itself, forms, at the end of a certain time, a considerable portion of sugar, which is crystallizable, and much resembling that of M. Kirchoff. This change takes place at a temperature between 68° and 77° of Fahrenheit, with or without access of air. There is also produced, at the same time, a gum possessed of properties analogous to that procured by roasting starch, and a peculiar substance which M. de Saussure calls *Amidine*. There is also formed a body, insoluble in water and in most acids, but which agrees with starch in forming a blue compound with iodine.

When the air has free access in these experiments, water is abundantly formed, carbonic acid is evolved, and a portion of charcoal is deposited. When the solid contents of this solution are examined, they are found greatly inferior in weight to that of the original starch. The loss is referred principally to the formation of water, and only in small part to the carbon carried off in the form of carbonic acid.

When air is excluded, no water is produced. A little carbonic acid and nearly pure hydrogen are evolved, and no carbonaceous deposit ensues. Whether the presence or absence of air influences the production of sugar, the author has not been able to determine.

The particular characters of the gum amidine, and other products of the fermentation of starch, are described at length in notes annexed to this paper; in one of which the author remarks, that the fixation of the elements of water, in the treatment of animal and vegetable substances by the common principles of the laboratory, occurs more frequently than is generally believed; and shows, by a comparative analysis of hog's lard in its recent state and after saponification, that the new properties which oils and fats acquire by saponification, is chiefly referable to the fixation of the elements of water.

On Corpora Lutea. By Sir Everard Home, *Bart. V.P.R.S.* Read January 14, 1819. [*Phil. Trans.* 1819, *p.* 59.]

In this paper the author describes the origin, growth, use, and decay of the Corpora lutea. The ovarium, before puberty, is a loose, open texture, in which are a number of globular cells. After puberty, the Corpus luteum forms in the substance of the ovarium. In the cow it appears, when magnified, as a mass of convolutions, somewhat like the brain. Sir Everard then proceeds to describe the drawings which accompany this paper, and of which the object is to show that the Corpora lutea are the structures in which the ova are formed; that they exist previous to, and perfectly independent of, sexual intercourse; and that, when they have fulfilled their office of forming ova, they are destroyed by absorption, whether the ova are impregnated or not.

On examining the appearance of the Corpora lutea before and after impregnation, it appears probable that impregnation is necessary for the expulsion of the ovum; but when impregnation does not take place, the ovum appears to remain in the cavity of the Corpus luteum. Hence it may be concluded, that impregnation takes place in the ovarium itself.

Remarks on the Probabilities of Error in Physical Observations, and on the Density of the Earth, considered, especially with regard to the Reduction of Experiments on the Pendulum. In a Letter to Capt. Henry Kater, *F.R.S. By* Thomas Young, *M.D. For. Sec. R.S.* Read January 21, 1819. [*Phil. Trans.* 1819, *p.* 70.]

In the first section of this letter, Dr. Young proceeds to examine in what manner the apparent constancy of many general results, subject to numerous causes of diversity, may be best explained; and shows that the combination of many independent causes of error, each liable to incessant fluctuation, has a natural tendency, dependent on their multiplicity and independence, to diminish the aggregate variation of their joint effect; a position illustrated by the simple case of supposing an equal large number of black and white balls to be thrown into a box, and 100 of them to be drawn out at once or in succession; when it is demonstrated that there is 1 chance in $12\frac{1}{2}$;

that exactly 50 of each kind will be drawn, and an even chance that there will not be more than 53 of either; and that it is barely possible that 100 black, or 100 white, should be drawn in succession.

From calculations contained in this paper, Dr. Young infers that the original conditions of the probability of different errors do not considerably modify the conclusions respecting the accuracy of the mean result, because their effect is comprehended in the magnitude of the mean error from which these conclusions are deduced. The author also shows that the error of the mean, on account of this limitation is never likely to be greater than six sevenths of the mean of all the errors divided by the square root of the number of observations.

The author then proceeds to the application of the doctrine of chances to matters of literature and history. He shows that with respect to the relation of two languages, nothing can be inferred from the coincidence of the sense of any single word; that the odds would be 3 to 1 against the agreement of two words; but if three were identical, it would then be more than 10 to 1 that they were derived from the same parent language. Six words give 1700 chances to 1, and eight near 100,000; so that, in these last cases, the evidence would approach certainty.

In regard to history, Dr. Young remarks, that the mention of a single number found indisputably correct may afford strong evidence of the veracity of a historian.

There is a manuscript of Diodorus Siculus, in which, describing the Egyptian funerals, he gives forty-two for the number of persons who sat in judgement on the merits of the deceased; and in a multitude of ancient rolls of papyrus, lately found in Egypt, forty-two assistants of Osiris are delineated on a similar occasion. Hence it is 100 to 1 that this manuscript is more accurate than others which have been collated; that Diodorus Siculus was a faithful historian; that the inscriptions related to some kind of judgement; and that the hieroglyphics have been truly interpreted.

The second section of Dr. Young's letter relates to the mean density of the earth.

Before we admit that the excess of density of the central parts of the earth, compared with its superficies, renders it probable that the whole was once fluid, we should inquire into the exclusive effect of pressure in augmenting the mean density. From this inquiry, Dr. Young makes it evident, that the general law, of a compression proportionate to the pressure, is amply sufficient to explain the greater density of the interior of the earth; and that this law, which is true for small pressures in all substances, and with regard to elastic fluids, in all circumstances, requires some little modification for solids and liquids, the resistance in them increasing somewhat faster than the density; for no mineral substance is light and incompressible enough to afford a sphere as large as the earth, and of the same specific gravity, without some such deviation from the general law. A sphere either of water or of air would be much denser: indeed the moon,

if perforated and containing deep cavities, would soon have absorbed her atmosphere, supposing she ever had one.

The author's letter concludes with some remarks on Euler's formula for the rolling pendulum, from which the perfect accuracy of Laplace's theorems for the length of the convertible pendulum rolling on equal cylinders may be inferred, without any limitation of their magnitude, or of the form of the pendulum. It also affords the proper correction for the arc of vibration.

On the Anomaly in the Variation of the Magnetic Needle as observed on Ship-board. By William Scoresby, *jun. Esq. Communicated by the Right Hon. Sir* Joseph Banks, *Bart. G.C.B. P.R.S.* Read February 4, 1819. [*Phil. Trans.* 1819, *p.* 96.]

The anomalies in magnetic observations on ship-board were usually attributed to imperfections of the azimuth compass, till Capt. Flinders suggested the influence of the iron used in the construction of the ship as their probable source,—a suggestion since confirmed and illustrated by Mr. Bain.

In this paper, Mr. Scoresby has given a table of the selected results of his observations relating to this subject, conducted on the coast of Spitzbergen, in the years 1815 and 1817. To these he has added some general inferences, deduced at the time of observation, and observes, that the anomaly of attraction is probably the greatest in men-of-war, and ships containing large quantities of iron, though it also exists to a considerable extent in merchantmen where iron forms no part of the cargo, especially in high latitudes where the dip of the needle is great.

On the Genus Ocythoë; *being an Extract of a Letter from* Thomas Say, *Esq. of Philadelphia, to* William Elford Leach, *M.D. F.R.S.* Read February 4, 1819. [*Phil. Trans.* 1819, *p.* 107.]

After describing a genus of Ocythoë, which the author regards as new, he observes that the Ocythoë offers a deviation from the ordinary laws which apply to the testaceous Mollusca, inasmuch as it resides only in the last volution of the shell; and as the shell does not fit the body, it appears probable that it was not made for it, more especially as there is no attachment between the shell and any part of the body. The shells that approach nearest to Argonauta are of that order: this supposition, however, is not corroborated by the habits of the animal; for all hitherto discovered of that order swim to the surface; and having no other organs of locomotion than fins, cannot glide upon the bottom. We must, therefore, suppose this to have been the habit of the animal; and yet it is hardly admissible that in that case it should have eluded the observation of voyagers, when the shell has often been found occupied by the parasite.

On Irregularities observed in the Direction of the Compass Needles of H. M. S. Isabella and Alexander, in their late Voyage of Discovery, and caused by the Attraction of the Iron contained in the Ships. By Captain Edward Sabine, *of the Royal Regiment of Artillery, F.R.S. &c.* Read February 18, 1819. [*Phil. Trans.* 1819, *p.* 112.]

In this paper Captain Sabine shows in what respect the effects of local attraction in the above-mentioned ships were conformable to observations made in previous voyages; and how far the errors found to take place on different courses, and under different dips of the magnetic needle, corresponded with those rules for calculating corrections recommended by Captain Flinders, who found that in every ship a compass would differ very materially from itself on being removed from one place to another, and this was found to be the case in the Isabella and Alexander.

As the ships ascended Davis's Straits, the binnacle compasses, in consequence of their construction, became nearly useless; accordingly, a standard compass was placed in the Isabella exactly amidship between the main and mizen mast, on a stout cross-beam, about nine feet above the deck; and in the Alexander amidship, on a box of sand five or six feet above deck. Captain Sabine next describes the methods by which the points of no error in these compasses were determined, and which were not in either ship coincident with the magnetic meridian.

Captain Flinders has shown that the maximum of error in the same compass, and confined to the same spot, is different in different parts of the world; and by multiplying the observations, and comparing the series, he was led to trace a connection between the amount of the errors and the dip of the needle, observing that the influence of local attraction on the compass needle increased with the dip. This increase, however, says the author, was a relative one, being in comparison to the directive power of magnetism, the diminution of which is sufficient to account for the effects observed; as will be evident upon reflecting, that though the magnetic force is greatest at the pole, its directive power must there have ceased: hence the inadequacy of the rule proposed by Captain Flinders, whereby the amount of error under any known dip being ascertained, the amount of error for any other dip may be calculated, by using as a multiplier the decimal expression of the proportion which the error in the one ascertained instance may have borne to the dip. In the observations made in the Isabella at Shetland, where the dip is 74° 21½′, the maximum of error was 5° 34′ easterly of the true variation, with the ship's head at E.S.E., and 5° 40′ westerly at W.N.W., making an extreme difference of 11° 20′. By Captain Flinders's rule, the common multiplier for this compass would have been about one twelfth, making the extreme difference 15°, whereas it was really more than 10°. By a similar reference to the observations made by the Alexander in Baffin's Bay, another proof is afforded of the inadequacy of Captain Flinders's rule.

Some Observations on the Formation of Mists in particular Situations. By Sir H. Davy, *Bart. F.R.S. V.P.R.I.* Read February 25, 1819. [*Phil. Trans.* 1819, *p.* 123.]

The author shows, in this paper, that after sunset the fall of temperature that ensues upon the earth's surface is considerably greater on land than in water, and refers to the well-known peculiarity in the expansibility of water, at temperatures below 40°, for the cause of its superior temperature and that of the air above it. When, therefore, the cold and comparatively dry air of the land mixes with the warmer and damper air that rests upon the water, the diminution of the temperature of the latter, occasioned by this mixture, tends to separate a portion of its moisture, and consequently to produce mist.

Observations on the Dip and Variation of the Magnetic Needle, and on the Intensity of the Magnetic Force; made during the late Voyage in search of a North-west Passage. By Captain Edward Sabine, *of the Royal Regiment of Artillery, F.R.S. and F.L.S.* Read February 25, 1819. [*Phil. Trans.* 1819, *p.* 132.]

The dipping-needle used in these observations was similar to that described by Mr. Cavendish in the 66th volume of the Philosophical Transactions, and was made by the same artist. It was so adjusted that no alteration took place in the indication of the dip on reversing the poles, and was placed in the direction of the magnetic meridian by a compass stationed at a sufficient distance, and suffered to remain during the observations, for the purpose of occasional verification.

In determining the intensity of the magnetic force, the needle was drawn to a horizontal position by a magnet, and, being released at an observed moment of time, was suffered to oscillate until the arcs became too small to be readily distinguished: the first arc was thus equal to the dip, and at every tenth vibration both the arc and time were noted. The results of these observations are given in a series of tables.

The azimuth compasses used in the observations to determine the variation of the needle in Davis's Strait and Baffin's Bay, and the results of which are detailed in tables annexed to this paper, were constructed upon Captain Kater's improved plan. They were generally employed on the ice; for as the influence of the ship's iron increased upon their compasses as the directive power of magnetism diminished, the observations made on board became of little or no value towards a knowledge of the true variation. This remark Captain Sabine illustrates by the insertion in his tables of a few azimuths taken in the Isabella. They also show how essential it is to navigation, in high latitudes, that the nature of the errors which the ship's attraction produces on her compasses should be understood.

On the Action of Crystallized Surfaces upon Light. *By* David Brewster, *LL.D. F.R.S. Lond. and Edinb. In a Letter addressed to the Right Hon. Sir* Joseph Banks, *Bart. G.C.B. P.R.S. &c. &c.* Read February 25, 1819. [*Phil. Trans.* 1819, *p.* 145.]

It has been remarked by Malus, that the action which the first surface of Iceland spar exercises upon light, is independent of the position of its principal section; that its reflecting power extends beyond the limits of the polarizing forces of the crystal: and that as light is only polarized by penetrating the surface, the forces which produce extraordinary refraction begin to act only at this limit. He also remarks, that the angle of incidence at which this spar polarizes light by partial reflection is 56° 30′; and that, whatever be the angle comprehended between the plane of incidence and the principal section of the crystal, the ray reflected by the first surface is always polarized in the same manner.

Dr. Brewster's experiments detailed in this paper lead him to opposite conclusions, and indicate an extension of the polarizing forces *beyond* the crystals. He also shows that the force of double refraction and polarization emanates from the surface of bodies, though its intensity depends upon the inclination of the surface to the axis of the crystal; and that the ordinary or extraordinary image may be extinguished at pleasure in any doubly-refracting crystal, which is thus converted into a singly-refracting crystal; that the change in the angle of polarization, produced by the interior force, depends on the inclination of the reflecting surface to the axis of the crystal, and upon the azimuthal angle, which the plane of reflection forms with the principal section; and that the change in the direction of the polarization depends upon the angle which the incident ray forms with the axis of the crystal.

On the Specific Gravity and Temperature of Sea-Waters, in different Parts of the Ocean, and in particular Seas; with some Account of their Saline Contents. *By* Alexander Marcet, *M.D. F.R.S. &c.* Read May 20, 1819. [*Phil. Trans.* 1819, *p.* 161.]

After some preliminary remarks upon the labours of others in this department of inquiry, Dr. Marcet proceeds to the immediate objects of his own investigation, which were to ascertain the specific gravity of many specimens of sea-water from different parts, and afterwards to examine their saline contents. The results relating to the first of these objects are prefaced by an account of the mode of taking the specific gravities, and of the instrument by which the water was raised, and of which two plates are annexed. The author then proceeds to the inferences deducible from his experiments, which, for the sake of conciseness, are given in the form of tables; whence it appears that the ocean in the Southern hemisphere is rather more salt than in the Northern, in the proportion of 1029·19 to 1027·57. The mean specific gravity of specimens from various parts of the

equator, is 1027·77, and therefore a little exceeds that prevalent in the Northern hemisphere, while it is decidedly less than that of the Southern Ocean. There is no material difference between different east and west longitudes at the equator, nor in the same hemisphere. In general, the salt seems most abundant in the deepest water, and in that furthest from land; the vicinity of ice also diminishes the saltness: if therefore this quality should increase in approaching the pole, it may be considered as militating against the probability of the ocean being extensively frozen in those regions. In general, small inland seas, communicating with the ocean, are less salt than the ocean itself. The waters of the Mediterranean, however, are more saline, and in attempting to account for this circumstance, the author's attention is directed to the relative densities of water from different depths of the ocean; and though in some cases it appeared lightest at the surface, such a result was generally referable to the vicinity of thawing ice, for, in ordinary circumstances, the density at great depths in no instance exceeded the mean density of the waters of the ocean. In regard, however, to different seas or arms of the ocean, the case is often different, in consequence of local circumstances. At the entrance of the Dardanelles, for instance, the difference between the upper and lower strata is as 1020 to 1028.

Dr. Marcet next details some researches upon the congelation of sea-water which takes place at 28°, and when slowly effected, is always attended by the total separation of the salt, which forms a strong brine that is washed away by the neighbouring water. The different specimens of water obtained from the ice in the late Northern Expedition, varied very little in specific gravity; the water was sweet, and in many instances its specific gravity did not exceed that of distilled water.

The second section of the author's paper relates to the saline contents of different seas. In his experimental inquiries connected with this subject, his objects were to ascertain the quantity of saline matter in a known weight of the water, and to compare it with the specific gravity: to precipitate the muriatic acid from a known weight of the water by nitrate of silver; the sulphuric acid by nitrate of barytes; the lime by oxalate of ammonia; and the magnesia, from the clear liquor remaining after the separation of the lime, by phosphate and carbonate of ammonia. The soda is inferred from calculation. The details of these experiments are thrown into a tabular form; whence it appears that sea-water, however different in specific gravity, contains the same ingredients all over the world, and that these bear nearly the same proportions to each other; the difference in specific gravity is therefore dependent upon the whole quantity of saline matter which they contain. The author concludes this communication by announcing the discovery of traces of potash in sea-water by Dr. Wollaston. He detected it by the addition of muriate of platina to the water, after evaporation to about one twentieth part.

An Account of the Fossil Skeleton of the Proteo-Saurus. By Sir Everard Home, *Bart. V.P.R.S.* Read March 4, 1819. [*Phil. Trans.* 1819, *p.* 209.]

After reverting to the contents of his former papers on this subject, Sir Everard describes in the present communication a specimen of the animal nearly in an entire state, and of which most of the parts are in such good preservation, as to enable him to correct and complete his former accounts, the only parts wanting being the bones of the pelvis and the lower part of the sternum. A drawing of the natural size accompanies this paper, in which, says the author, the parts are so clear and distinct, as to render any verbal description superfluous.

Reasons for giving the name Proteo-Saurus to the Fossil Skeleton which has been described. By Sir Everard Home, *Bart. V.P.R.S.* Read April 1, 1819. [*Phil. Trans.* 1819, *p.* 212.]

The specimen of the fossil skeleton, described in the author's last paper, having proved that the animal had four legs, and that its progressive motion through water is similar to that of fishes, he was led to look for its place in the scale of gradation between amphibia and fishes. With this view he examined the vertebræ of the Proteus, which he found cupped at both extremes, in which respect it resembles the fossil animal: it is also nearly allied to it in having feet; and were it not that the bones of the chest show that the lungs were more capacious, and that in the largest specimens there is not sufficient space between the occiput and first rib for gills, Sir Everard would have ventured to have called it Proteus; but as it is highly probable that this animal breathed by lungs only, and appears to have been capable of the two kinds of progressive motion, it may be called Proteo-saurus.

Some Observations on the Peculiarity of the Tides between Fairleigh and the North Foreland; with an Explanation of the supposed Meeting of the Tides near Dungeness. By James Anderson, *Captain in the Royal Navy. Communicated by the Right Hon. Sir* Joseph Banks, *Bart. G.C.B. P.R.S.* Read March 25, 1819. [*Phil. Trans.* 1819, *p.* 217.]

After adverting to the circumstances upon which the generally adopted opinion is founded, that the tides meet between Dungeness Point and Rye Harbour, Captain Anderson describes the peculiarity of the Channel at that point, and its very sudden contraction between Dungeness and Cape D'Alprèe, and between the South Foreland and Calais Point; so that the western tide meets with a resistance to its course at Dungeness and Cape D'Alprèe; where, from the passage being insufficient to discharge the water brought from the westward, it must accumulate until it deepens and widens the Chan-

nel, so as to become adequate to the discharge of the water. The peculiarities of the rise and fall of the tides at adjacent places, is referred by the author principally to the accumulation that takes place in these basins. That the tides do not meet at Dungeness in a line across the Channel, is further proved by the absence of that violent concussion of water which in such a case would ensue; the fact being, that the formation of the coast by gradually altering the course of the tide between the South Foreland and buoy of the Nore, from E.N.E. to W.N.W. within the stream of the Goodwin Sands, occasions a gentle blending of the waters, so that there is only a strong eddy about the Kentish Knock, and a foamy rippling where they meet and proceed onwards together.

On the Ova of the different Tribes of Opossum and Ornithorhynchus. By Sir Everard Home, *Bart. V.P.R.S.* Read March 25, 1819. [*Phil. Trans.* 1819, *p.* 234.]

With his previously acquired knowledge respecting the formation of the ova of quadrupeds in Corpora lutea, Sir Everard proceeds to inquire into that of the Opossum tribe, the ova of which are not formed in the same manner, but make two distinct gradations between the quadruped and *Ornithorhynchus paradoxus*, which last approaches so near to the bird, as to complete the link of gradation between the quadruped and bird in their mode of generation. Sir Everard first describes the formation of the ova in the Kangaroo, which, when expelled from the Corpus luteum, receive a yolk in the Fallopian tube, and afterwards the albumen in the uterus. The fœtus, when expelled from the uterus into the marsupium, attaches itself to the nipple, as described in the 85th and 100th volume of the Philosophical Transactions. In the Kola and Wombat, and great and small Opossum, instead of Corpora lutea there are yolk bags imbedded in the substance of the ovarium; and there are two uteri, with a Fallopian tube to each, the ovum in each uterus being separately impregnated in its own cavity.

The mode of formation of the ova in the Ornithorhynchi, forms the intermediate link between the Opossum and bird. The yolk bags are imbedded in the ovaria; and instead of a regular uterus, each Fallopian tube swells out into a cavity, in which the ova are impregnated.

The Results of Observations made at the Observatory of Trinity College, Dublin, for determining the Obliquity of the Ecliptic, and the Maximum of the Aberration of Light. By the Rev. J. Brinkley, *D.D. F.R.S. and M.R.I.A. and Andrews Professor of Astronomy in the University of Dublin.* Read April 1, 1819. [*Phil. Trans.* 1819, *p.* 241.]

The obliquity of the ecliptic, as deduced from the early observations by the Greenwich quadraut, compared with the present ob-

liquity, gives the diminution for an interval of nearly sixty years, with almost sufficient accuracy to state with some confidence the mass of Venus; but to obtain this point with certainty, the present obliquity, deduced from a mean of the observations of different astronomers, should be used. Upon this subject the author alludes to the opinion of astronomers, that observations of the winter solstice have given a less obliquity than those of the summer solstice,—an opinion sustained by the observations of Maskelyne, Arago, and Pond, but questioned by Bessel and Bradley. Dr. Brinkley refers this difference to some unknown modification of refraction; he has observed that at the winter solstice the irregularity of refraction for the sun is greater than for the stars at the same zenith-distance. He points out the necessity of paying attention to the observations at the winter solstice, and gives a table, exhibiting the mean obliquity reduced to January 1813.

Dr. Brinkley next alludes to the maximum of the aberration of light, which appears from his observations of last year to be 20″·80.

On some New Methods of investigating the Sums of several Classes of Infinite Series. By Charles Babbage, *Esq. A.M. F.R.S.* Read April 1, 1819. [*Phil. Trans.* 1819, *p.* 249.]

The object of this paper is to explain two methods of finding the sums of a variety of infinite series. One of these the author discovered several years ago; but finding that some of the results to which it led were erroneous, he then declined publishing it. In inquiring into the causes of these errors, he was led to the second method, which employs the process of integration relative to finite differences. The cause of the fallacies in the former method was afterwards discovered, and in this paper a criterion is proposed for judging of the truth of the results, and a mode of correcting them where found to be erroneous. The sums of a variety of series are found by these methods; and the author concludes by observing, that he has since been informed by M. Poisson, that that gentleman had arrived at some nearly similar results in investigating a problem in physical astronomy, and also that some investigations of a similar nature were found amongst the papers of Lagrange, but that neither of these mathematicians had explained the cause of the errors, or given a method of correcting them.

On the Optical and Physical Properties of Tabasheer. By David Brewster, *LL.D. F.R.S. Lond. and Edin. In a Letter to the Right Hon. Sir* Joseph Banks, *Bart G.C.B. P.R.S. &c.* Read May 6, 1819. [*Phil. Trans.* 1819, *p.* 283.]

Tabasheer is a substance found in the cavities of the bamboo, existing originally in the state of a transparent fluid, but gradually indurating into a solid of different degrees of hardness: it consists of 70 silica, + 30 potash and lime. One variety has a milky trans-

parency, transmitting a yellowish, and reflecting a bluish light; another is translucent, and a third opake: the two first varieties become transparent, and evolve air when immersed in water: the third evolves air also, but remains opake. If the first varieties be only slightly wetted they become quite opake. The property of acquiring transparency by the evolution of air from, and the absorption of water by its pores, belongs also to the hydrophanous opal; but the faculty of becoming opake by a small quantity, and transparent by a larger, of water, shows a singularity of structure in tabasheer. As the tabasheer disengages more air than hydrophane, its pores must be more numerous; and therefore the transmission of light, so as to form a perfect image, indicates either a very feeble refractive power or some peculiarity in the construction of its pores. To determine this, Dr. Brewster formed a prism of tabasheer with an angle of 34° 15′, and upon measuring its refractive power found it very low, though various in different specimens, the index of refraction varying from 1·11 to 1·18, that of water being 1·33, of flint-glass 1·60, of sulphur 2·11, of phosphorus 2·22, and of the diamond 2·47. So that tabasheer has a lower refractive power than any other solid or liquid, and holds an intermediate place between water and the gases. Dr. Brewster then gives a formula for computing the absolute refractive power of bodies, and a table of results, from which it appears that, in this respect, the refractive power of tabasheer is so low as to be separated by a considerable interval from all other bodies.

The author next proceeds to detail a variety of experiments upon the absorbent powers of the different kinds of tabasheer, in respect to several liquids, and the corresponding effects upon its optical properties and specific gravity, and concludes with observations on the cause of the paradox exhibited by the transparent tabasheer, in becoming opake by absorbing a small quantity of water, and transparent when the quantity is increased.

An Account of a Membrane in the Eye, now first described. By Arthur Jacob, *M.D., Member of the Royal College of Surgeons in Ireland, Demonstrator of Anatomy and Lecturer on Diseases of the Eye in the University of Dublin. Communicated by* James Macartney, *M.D. F.R.S.* Read July 1, 1819. [*Phil. Trans.* 1819, *p.* 300.]

In this paper the author describes a delicate transparent membrane, covering the external surface of the retina, and united to it by cellular substance and vessels. Its appearance varies in the different classes of animals, and at different ages. In young animals it is transparent and tender; but in the adult firm, and stained by the pigment. In fishes, it has been described by Haller and Cuvier as the medullary layer of the retina; but the author thinks incorrectly, since it presents no character of nervous structure, and the retina remains perfect before it. The author concludes this com-

munication by describing his mode of examining delicate anatomical structures :—He procures a hollow sphere of glass, between two and three inches in diameter, of which one fourth is cut off at the open part, and the edges ground so as to fit upon a plate of glass to which the object is attached and immersed in water; the sphere is then filled with water, and inverted over the object upon the plate. The whole being withdrawn from the basin the object may be examined, and the portion of the sphere filled with water furnishes a convenient magnifying power.

A New Method of Solving Numerical Equations of all Orders, by continuous Approximation. By W. G. Horner, *Esq. Communicated by* Davies Gilbert, *Esq. F.R.S.* Read July 1, 1819. [*Phil. Trans.* 1819, *p.* 308.]

The process which the author endeavours to establish in this essay, being the leading theorem in the calculus of derivations, presented under a new aspect, may, he says, be regarded as an universal instrument of calculations, extending to the composition as well as analysis of functions of every kind, but it promises to be especially useful in the numerical solution of equations.

Mr. Horner then proceeds to the illustration of his method, and to explain the investigations to which it is applicable, by details which do not admit of explanation.

An Account of Experiments for Determining the Variation in the Length of the Pendulum Vibrating Seconds, at the principal Stations of the Trigonometrical Survey of Great Britain. By Captain Henry Kater, *F.R.S.* Read June 24, 1819. [*Phil. Trans.* 1819, *p.* 337.]

In this communication Captain Kater, having noticed the circumstances to which his researches owe their origin, proceeds to detail his investigations, and to describe the implements and apparatus employed in his various inquiries; the construction of the pendulum and its appendages is minutely explained, as also the rate of its expansion for each thermometric degree, whence is deduced the corresponding correction to be applied to the number of its vibrations. The operations at each station, with their results, are enumerated at length, and illustrated by numerous tables. The length of the seconds pendulum for the latitude of London is 39·13722 inches in parts of the scale which forms the basis of the trigonometrical survey; for the latitude of Unst 39·16939 inches, of Portsay 39·15952, of Leith Fort 39·15347, of Clifton 39·14393, of Arbury Hill 39·14043, and of Shanklin Farm 39·13407 inches. The calculation of the latitude of each of these stations is given at length, to afford the opportunity of any further examination desirable on that subject; but these and the other details relating to calculation do not admit of abridgement.

Captain Kater concludes this paper with some observations re-

specting the figure of the earth. It having been shown by Clairaut that the sum of the two fractions, expressing the ellipticity and the diminution of gravity, from the pole to the equator, is always a constant quantity, and equal to $\frac{5}{2}$ of the fraction, expressing the ratio of centrifugal force, and that of gravity at the equator, it follows that if the decrease of gravity from the pole to the equator be subtracted from this constant quantity, the remaining fraction will express the ellipticity of the spheroid. The diminution of gravity may be known by finding the difference of the length of two pendulums, vibrating in equal times at the equator and pole, which are to each other directly as gravitation; but as such experiments cannot be made at the pole, Captain Kater proceeds to describe the means of obtaining the desired result by observations at intermediate stations; whence it appears that the length of the seconds pendulum at the equator, deduced from the observations at Unst and Dunnose, is 39·00527 inches, and gravitation at the equator 16·040 feet; hence the centrifugal force at the equator is $\frac{1}{290}$ of gravitation, or $\frac{1}{289}$ of gravity, which last being multiplied by $\frac{5}{2}$ gives ·0086505 for the sum of the fractions, expressing the ellipticity of the earth and diminution of gravity from the pole to the equator.

It appears from this result that, excepting the allowance for height above the sea's level, the error in the number of vibrations of the seconds pendulum at any particular station does not amount to $\frac{1}{10}$th of a vibration, which is about equal to 400,000th part of the length, consequently gravitation may be determined to this degree of accuracy by the apparatus employed; and in passing through a country composed of materials of various degrees of density, the pendulum may be expected to indicate such variations with much precision. Irregularities that are observed in the decrease between given latitudes, from the pole to the equator, are referable to this irregular attraction; thus the sudden increase of gravitation at Arbury Hill is referred by Captain Kater to the granite of Mount Sorrel in Leicestershire.

Though the details of M. Biot's experiments are not yet published, the author observes that it affords him no small gratification to learn that the acceleration of the pendulum between London and Unst, computed by that mathematician from his observations between Unst and Formentara, differs only 0″·6 from the result of his own experiments,—a difference referable perhaps to the superior density of Unst compared with that of the substrata of London.

The Croonian Lecture. A further Investigation of the Component Parts of the Blood. By Sir Everard Home, *Bart. V.P.R.S.* Read November 4, 1819. [*Phil. Trans.* 1820, *p.* 1.]

In this communication Sir Everard announces the existence in certain animal structures of globules smaller than and independent of those ordinarily belonging to the blood; they were first remarked by Mr. Bauer, during the microscopic examination of the

coats of an aneurismal tumour; their number was in the proportion of one to four of the larger globules, and their diameter was $\frac{1}{18000}$ of an inch in the larger in contact with the circulating blood; in the other layers they became more numerous, and in that longest coagulated were in the proportion of four to one.

In the section of an aneurismal tumour, the author notices the uncommon appearance of crystals of sulphate of lime with muriate and phosphate of soda, which, as well as the globules, he supposes to have been originally dissolved in the serum, since they are only brought to view by the act of coagulation.

In coagulable lymph deposited during violent inflammation, the same small globules were observed, mixed with a few colourless blood globules. The globules, stated by the author in a former lecture to have been produced in the serum, are also of a similar nature; and to distinguish them from the larger blood globules, he proposes to call them globules of lymph. In the buff of blood they were very numerous in the upper and firmest part, but the lower and softer layer consisted principally of blood globules.

A tumour in the prostate gland was found Mr. Bauer to contain both kinds of globules, and a considerable proportion of transparent jelly. The hard compact part of a tumour in the breast consisted chiefly of lymph globules and elastic jelly. In its softer part the blood globules predominated, so that the structure of these tumours is nearly allied to that of aneurismal sacs.

In buffy blood the proportion of aëriform matter, evolved under the exhausted receiver of the air-pump, was less than that of healthy blood, and appeared in excess in the blood drawn from the arm of a person an hour after a hearty meal. In the mucus of the pylorus and duodenum Mr. Bauer found lymph and blood globules. In the chyle he found the size of the globules various; those which were very small increased in size whilst under the microscope, and became as large as blood globules enveloped in their colouring matter; they appeared opake and milk white. Mr. Bauer is satisfied, from these observations, that the globules of the blood are perfectly formed in the mesenteric glands, with the exception of colour, which they receive in the vessels of the lungs.

The Bakerian Lecture. On the Composition and Analysis of the Inflammable Gaseous Compounds resulting from the Destructive Distillation of Coal and Oil, with some Remarks on their relative Heating and Illuminating Powers. By William Thomas Brande, *Esq. Sec. R.S. Prof. Chem. R. I.* Read November 18, 1819. [*Phil. Trans.* 1820, *p.* 11.]

This paper is divided into two sections: in the first, the author's object is to show that no other compound of carbon and hydrogen can be demonstrated to exist except that usually termed *olefiant gas*, consisting of one proportion of carbon and one of hydrogen; and that the supposed compound of one of carbon and two of hydrogen, gene-

rally called *light hydrocarbonate*, is in reality a mere mixture of hydrogen and olefiant gases. In proof of this opinion he details a series of analytical experiments upon the gases from coal, oil, acetate of potash, moist charcoal, &c., conducted chiefly by detonation with oxygen, by heat alone, and by the action of sulphur at high temperatures, and obtains results analogous to those afforded by mixtures of hydrogen and olefiant gas, of the same specific gravities. Of the gases above-mentioned, however, the specific gravity, combustibility, and intensity of light during combustion, are often much interfered with by the presence of carbonic oxide and carbonic acid.

Of the products obtained by the destructive distillation of coal and oil, Mr. Brande thinks that some are of what may be termed secondary formation; that is, that they result from the mutual action of the first formed gaseous products at high temperatures. Thus a peculiar compound of hydrogen and carbon, volatile and odorous, resembling tar in appearance, but having the characters of resin, is formed by passing pure olefiant gas through a tube of red-hot charcoal; and sulphuret of carbon is formed by the mutual agency of carburetted and sulphuretted hydrogen gases at high temperatures. To the latter compound the author refers the production of sulphurous acid, by the combustion of coal gas in cases where, by the test of acetate of lead, it is shown to be free from sulphuretted hydrogen. In this section of the paper the author further details some processes for the analysis of complex gaseous mixtures, which he thinks afford more accurate results, and are easier of performance than those usually practised, and which are rendered important as elucidating the nature of the gaseous products, now in common use for artificial illumination.

The second section of this lecture contains comparative experiments on the heating and illuminating powers of olefiant, coal, and oil gases, with some incidental remarks on the general properties of radiant matter.

To give the light of ten ordinary wax candles for one hour it required 2600 cubic inches of olefiant gas, 4875 of oil gas, and 13,120 of coal gas. The consumption of oxygen amounts in the first to 7800 cubic inches, in the second to 11,578 cubic inches, and in the third to 21,516; hence it follows that the heating powers of the respective flames being nearly similar, there will necessarily be much more heat produced by coal gas illumination than by an equal illumination of oil gas, and by oil gas more than by olefiant.

The economy of combining several flames for the production of light is strikingly illustrated by comparative experiments upon insulated and combined jets of flame. Thus, in respect to olefiant gas, a single jet, producing the light of one candle, consumes 640 cubic inches per hour; while a burner, with united jets of flame, giving the light of ten single jets, consumes only 2600 cubic inches, instead of the estimated quantity of 6400.

Some comparative experiments upon the effects of solar and terrestrial light conclude this lecture, from which it appears that, al-

though the light of gas flames, concentrated into a very brilliant focus by means of thick plano-convex lenses, produced a very sensible heating power; it does not in the slightest degree blacken the chloride of silver, nor does it influence the combination of chlorine with hydrogen, which is a yet more susceptible test of the direct influence of the solar rays; on the other hand, the brilliant light occasioned by the discharge of the voltaic apparatus, presently blackens the chloride of silver; and when a mixture of chlorine and hydrogen is exposed to its influence, it causes the production of muriatic acid, sometimes quietly and sometimes with explosion, in the manner of the solar rays. The concentrated rays of lunar light neither possess heating powers, nor do they appear to influence chemical combination.

In conclusion the author, after adverting to the inefficiency of Mr. Leslie's photometer, for measuring the intensity of artificial light, suggests an instrument in which the effects are measured by the expansion of the vapour of ether, renewable from a column of that fluid.

On the Elasticity of the Lungs. By James Carson, *M.D. Communicated by* Thomas Young, *M.D. For. Sec. R.S.* Read Nov. 25, 1819. [*Phil. Trans.* 1820, *p.* 29.]

In a treatise published some years ago on the motion of the blood, the author contended that the influence of the elasticity of the lungs upon that function had been overlooked by physiologists. The object of the present communication is to ascertain the real force of this elastic power, as it exists in the healthy living body. For this purpose Dr. Carson connected with the trachea of several animals a glass syphon, so placed as to admit of pressure being exerted upon the lungs by a column of water contained in it; an opening was made into the cavity of the chest on both sides, and the height of the column of water in the tube was considered as equivalent to the pressure exerted upon it by the elastic power of the lungs of an ox: the author thinks it clearly ascertained "that the spring of air compressed by a column of water, of a foot and a half high, is not equal to the rebounding spring of the lungs at the usual stage of their dilatation." The only experiment, however, which gave, in the author's opinion, conclusive results, was made upon a dog; for in all the others the gradual sinking of the water to its ordinary level in the syphon indicated some wound in the lungs. In the present case the height of the column of water supported in the tube was ten inches.

Dr. Carson concludes this paper with some observations on the modes of effecting artificial respiration, and on the method of ascertaining the actual quantity of air existing in the lungs after complete expiration.

On the Action of Crystallized Bodies on Homogeneous Light, and on the Causes of the Deviation from Newton's Scale in the Tints which many of them develope on Exposure to a Polarized Ray. By J. F. W. Herschel, *Esq. F.R.S. Lond. and Edin.* Read December 23, 1819. [*Phil. Trans.* 1820, *p.* 45.]

When Malus published his discovery of the polarization of light, the list of doubly-refracting crystals was small; and as the most remarkable of them possessed only one axis of double refraction, it was presumed that the law discovered by Huyghens, applicable to that one, might hold good in all; but the discovery of crystals with two axes of double refraction has proved the fallacy of such generalization, and rendered new and extensive investigations necessary.

There are two modes of conducting observations on double refraction and polarization; the one turns upon immediate observation of the angular deviation of the extraordinary pencil, the other depends upon the separation of a polarized ray into complementary portions by the action of a crystallized lamina. After noticing the advantages of the latter, Mr. H. observes, that to render observations on the tints developed by polarized light available, they must be comparable to each other; hence the importance of discovering the existence and tracing the laws of those causes which operate to disturb their regularity. In the author's first inquiries on the polarization of light, he was struck by the great deviation from the succession of colours in their laminæ, as observed by Newton, which many crystals exhibit when cut into plates perpendicular to one of their axes; and finding this phenomenon unconnected with irregularities in their thickness or polish, and uniformly repeated in different and perfect specimens, he was led to inquire into their causes, especially as they appeared to form an unanswerable objection to M. Biot's theory, which perfectly explains the tints in crystals with one axis.

In the several sections of this communication, Mr. H. first enters into a detailed description of the phenomena themselves, which are reducible to one general fact; namely, that the axes of double refraction differ in their position in the same crystal for the different coloured rays of the spectrum, being dispersed in one plane over an angle more or less considerable according to the nature of the substance. In many bodies, the magnitude of this dispersion of the axes is comparatively trifling; while in some, not otherwise remarkable for a high ordinary or extraordinary dispersive power, it is enormous, and renders all computation of the tints in which it is not taken into consideration completely erroneous. A new element is thus developed, which for the future must enter into all rigorous formulæ of double refraction; and another striking instance is presented of the inherent distinction between the different coloured molecules of light. At the same time, says the author, by the complete explanation this principle affords of all the more perplexing

anomalies in the tints, the theory of oscillations stands relieved of every difficulty, and may be received as adequate to the representation of all the phenomena of the polarized rings, and entitled to rank with the fits of easy transmission and reflection as a general and simple physical law.

A Case of the Human Fœtus found in the Ovarium, of the Size it usually acquires at the End of the Fourth Month. By A. B. Granville, *M.D. F.R.S. In a Letter addressed to Sir* Everard Home, *Bart. V.P.R.S.* Read January 13, 1820. [*Phil. Trans.* 1820, *p.* 101.]

The body, of which the dissection is described in this paper, exhibited a considerable tumour, placed immediately above the region of the pubis. Upon opening the abdomen, a quantity of fluid resembling blood was found in its cavity, and a tumour, four times the size of a hen's egg, obstructed the view of the internal parts of generation, resting upon the left portion of the anterior surface of the uterus; a blood-vessel, which proved to be a branch of the left spermatic artery, and of the size of a large crow-quill, penetrated the dense portion of this tumour, and a smaller vessel communicated with the spermatic vein. The inferior portion of the tumour presented diaphanous membranes, through which was seen a fœtus of about four months' growth.

The fact of an extra-uterine conception being thus made out, the author proceeded to examine the connection of the tumour with the neighbouring parts, and the condition of the uterus; whence it appears that the tumour was seated in the left ovarium, which had burst in three places, so as to suffer the membranous sac containing the fœtus to protrude into the cavity of the abdomen; and that by the growth of the fœtus that part of the covering of the ovarium was ultimately lacerated, which involved the placenta by which the adhesions of the latter were torn, producing the sudden and fatal hæmorrhage which killed the patient and filled the abdomen with blood.

The uterus had acquired considerable development during the period of ovario-gestation; the right ovary was healthy, as also its Fallopian tube. The left Fallopian tube was sound and loose, and its fimbriated extremity had no connexion with the neighbouring tumour.

Many other circumstances relating to the history of this case, and to the anatomical structure exhibited by the disputed parts, are contained in an explanation of Mr. Bauer's drawings, which illustrate and accompany the paper.

On some Combinations of Platinum. By Edmund Davy, *Esq. Professor of Chemistry, and Secretary to the Cork Institution. Communicated by* F. Babington, *M.D. F.R.S.* Read February 17, 1820. [*Phil. Trans.* 1820, *p.* 108.]

The principal object of this paper is to describe a peculiar compound of platinum, obtained from the sulphate by the agency of alcohol. On boiling sulphate of platinum with alcohol, a substance is precipitated, which, when dried, is black, insoluble in water, and unalterable by exposure to air. It is reduced with a slight explosion when heated. It acquires fulminating properties when steeped in ammonia. It is insoluble in nitric, sulphuric, and phosphoric acids, and slowly soluble in muriatic acid. By alcohol it is immediately decomposed, as shown by slightly moistening it with that liquid; and such heat is produced as to ignite the separated platina. During these changes acetic acid is formed.

Mr. Davy gives a detailed analysis of this powder, whence it appears to consist almost solely of platinum, with a little oxygen, and the elements of nitrous acid; a small portion of carbon was also obtained, which, however, the author regards as accidental; the presence of nitrous acid is referred to the manner in which the sulphate of platinum is formed, namely, by the agency of nitrous acid on sulphuret of platinum.

In the fifth and sixth sections of this paper, Mr. Davy describes the action of sulphate of platinum upon solutions of jelly, in which it forms a precipitate composed, when dried at 212°, of

56·11 oxide of platinum,
20·02 sulphuric acid,
23·87 gelatine and water.

The author considers the sulphate of platinum as the most delicate known test for jelly.

In the seventh section of his paper, Mr. Davy describes a grey oxide of platinum, obtained by the action of nitrous acid on fulminating platinum, and affording on analysis,

$$100 \text{ platinum} + 11{\cdot}9 \text{ oxygen}.$$

Assuming, with Vauquelin and Berzelius, that the black oxide of platinum contains 15 per cent. of oxygen, the author observes that the grey oxide which he has described may be considered as the protoxide, and will consist of one proportion of platinum and one of oxygen, or 126 platinum + 15 oxygen, and the black oxide will consist of 126 platinum and 22·5 oxygen, or of one proportion of metal and $1\frac{1}{2}$ of oxygen.

On the Methods of Cutting Rock Crystal for Micrometers. By William Hyde Wollaston, *M.D. F.R.S.* Read February 24, 1820. [*Phil. Trans.* 1820, *p.* 126.]

For the purpose of examining the phenomenon of double refraction, it is easy to combine a wedge of rock crystal with one of crown

glass, so that a luminous object seen through them shall appear in its true place by ordinary refraction, accompanied by a second image produced by extraordinary refraction.

In consequence of the dispersion of colours which occurs in employing different substances, such a combination is not suited for the micrometer invented by Abbé Rochon; but it is not difficult to obtain such a section of rock-crystal as may be substituted for the glass wedge, so that the pencil of light shall be colourless without diminishing the separation of the images. But since the degree to which the double refraction of rock crystal separates the two portions of a beam of light transmitted through it, is sometimes not sufficiently great, it becomes desirable to increase it; and though the means of effecting this have not been described, the author proceeds to explain the method that he has found advantageous, and which he regards the same as that of M. Rochon.

The author then describes three modes of cutting wedges of rock crystal, the axis of crystallization being differently placed in each. In the first, or horizontal wedge, the axis is at right angles to the first surface. In the second, or lateral wedge, the axis is in the first surface and parallel to its acute edge. In the third, or vertical wedge, the axis is also in the first surface, but at right angles to the acute edge. An object seen through the first wedge, in the direction of the axis, does not appear double; but in the others the transmitted rays pass at right angles to the axis, and they each produce two images.

By placing two of these wedges together, with their acute edges in opposite directions, there are obviously three modes in which they may be combined in pairs, represented by LH, VH, and VL. In the two first cases, the separation of the images will be the same, and an object seen through the combination appears double to the amount of 17′; but the third produces a distinct effect; for, by reason of the transverse position of the axes of crystallization, the separation of the two images becomes exactly doubled. The pencil ordinarily refracted by the first wedge is refracted extraordinarily by the second, and *vice versâ*, so that neither of the divided pencils returns to its true place; and since one falls as much short of the mean as the other exceeds the truth, they are ultimately separated twice the usual distance between the ordinary and extraordinary refractions, and thus the images are separated 34′. This, the author says, it can scarcely be doubted is essentially the construction employed by M. Rochon.—This paper is concluded by some further directions respecting the mode of cutting and arranging the prisms for the above purpose.

On a New Principle of constructing Ships in the Mercantile Navy. By Sir Robert Seppings, *F.R.S.* Read March 9, 1820. [*Phil. Trans.* 1820, *p.* 133.]

In the present mode of constructing the ribs of English merchant ships, only half the timbers are united, so as to constitute any part

of an arch, every alternate couple only being connected together; the intermediate two timbers being unconnected, and resting upon, instead of supporting, the outer planking. The mode of joining the different pieces of the same rib is also highly objectionable. It is effected by the introduction of a wedge-piece, by which the grain of the rib pieces is much cut, and the general fabric weakened, with a great consumption of materials. The object of the introduction of these *wedge pieces* or *chocks*, is to procure the curvature requisite in forming a ship, when crooked timber is scarce; but the curve may be equally obtained by a different arrangement of materials, and with less consumption of useful timber.

After pointing out several other defects and disadvantages arising out of the present mode of building mercantile ships, Sir Robert proceeds to consider the best means of obviating them. He employs shorter lengths of timber and of less curvature, consequently less grain-cut, and their ends are connected by coaks or dowels, instead of wedge pieces. In the event of a ship grounding, such a construction is much better adapted to give support and strength to the fabric than the former.

The advantages of this new principle in practice appear from a report of the officers of Woolwich Yard to the Navy Board, relating to a comparison of the Talavera, built upon the improved construction, with the Black Prince, constructed in the usual way. Another important circumstance relating to the Talavera is, that her frame consists of small timber, hitherto considered as only applicable to frigates, but which, when properly combined, may, in Sir Robert's opinion, be rendered equal in strength and economy to the large and often grain-cut materials used in the frames of large ships.

Several drawings accompany this paper, the inspection of which is requisite to render the further details which it contains intelligible.

On the Milk Tusks, and Organ of Hearing of the Dugong. By Sir Everard Home, *Bart. V.P.R.S.* Read April 13, 1820. [*Phil. Trans.* 1820, *p.* 144.]

The skull upon which the following observations were made, was sent to the author from Sumatra by Sir Thomas Stamford Raffles, and is the only perfect specimen in Europe. The milk tusks were first examined; and as their points only were visible, one of the bony canals in which the tusk is contained was laid open: it was removed from its socket, and cut through longitudinally, when it appeared similar to the milk tusks of the narwhale and elephant, and like them deficient in external smoothness as compared with the permanent tusks.

The milk tusk of the dugong is peculiar in having a shallow cup attached to its base, apparently for the purpose of receiving the point of the permanent tusk as soon as formed; and as the milk tusk advances in the act of being shed, the other may be directed forwards in the same course, which differs from that in which it set out.

The milk tusks of the dugong have hitherto been mistaken for its permanent tusks, the appearance of which is unknown, since no full-grown skull has hitherto been examined. The grinding teeth differ from those of any other known animal: they consist of a double cone, the external crust of which is not enamel, and covers an interior harder coat; the bulk of the tooth consisting of soft ivory, so that in wearing down, these teeth will be formed into a concave surface.

The organ of hearing presents a peculiarity, says the author, unknown in any other tribe of animals; the malleus and incus being fastened to the sides of the tympanum by a bony substance extending across the intervening space; the stapes is opposed to, but not connected with the foramen of the incus; the handle of the malleus projects in the centre of the circle over which the membrana tympani had been spread, so that in the perfect animal it is doubtless attached to the centre of the membrane.

The habits of the dugong being allied to those of the hippopotamus, Sir Everard was induced to examine how far this peculiarity of structure might exist in the latter animal; it is, however, perfectly different, the ossicula in the ear of the hippopotamus being separate, and readily dropping out of the cavity of the tympanum. The ossicula of the hippopotamus are small compared with the size of the animal; but the cochlea makes two turns and a half, which is uncommon. The semicircular canals present no apparent peculiarity. In the dugong the semicircular canals are extremely small, as is the cochlea, which only makes one turn and a half.

The peculiar bony union of the ossicula of the ear with the skull renders it probable that this animal is more indebted for its hearing, than any other that lives in the water, to the vibrations received by the bones of the skull being communicated to the ossicula, and thence to the cochlea and canals.

Upon the different Qualities of the Alburnum of Spring and Winter-felled Oak Trees. By Thomas Andrew Knight, *Esq. F.R.S.* Read April 20, 1820. [*Phil. Trans.* 1820, *p.* 156.]

It has long been believed that oak timber felled in winter is superior to that felled in spring; but the cause of the superiority has not been inquired into, and the practice of winter-felling has been discontinued, in consequence of the superior value of the spring bark.

In the spring of 1817, the author selected two oak-trees, not quite a century old, and growing near each other: the one was barked and suffered to remain standing; the other felled, and being immediately stripped of bark, was placed in a situation protected from sun and rain. The following December, the other tree, still alive, was cut down, placed in the same situation, and pieces of each, from similar parts, were subjected to the following experiments, at different subsequent periods.

The specific gravity of the spring-felled alburnum was 0·666; that of the winter-felled, 0·565. Equal blocks of each alburnum were cleaved into thin pieces; and, when perfectly dry, suspended in a damp room for ten days. One thousand grains of the alburnum of the spring-felled tree gained 162 grains, and of the winter-felled 145; so that there is an obvious difference in the properties of the two; and Mr. Knight doubts not, by taking the bark off in the spring, and not felling the tree till winter, that the timber would be materially improved. He also thinks that these observations are applicable to the heart wood as well as to the alburnum, though he has not at present any very conclusive evidence to offer on that subject.

On the Mode of Formation of the Canal for containing the Spinal Marrow, and on the Form of the Fins (if they deserve that name) of the Proteo-Saurus. By Sir Everard Home, *Bart. V.P.R.S.* Read May 4, 1820. [*Phil. Trans.* 1820, *p.* 159.]

The structure of the vertebræ of the Proteo-saurus is intermediate between that of the lizard tribe and cartilaginous fishes, and they have so close a resemblance to the vertebræ of the shark, as often to have been mistaken for them. They are composed of bone, and have a body and canal for the spinal marrow, and a process for the attachment of muscles; but the body is made up of one piece, while the spinal process, and two lateral branches which belong to it, are made up of another; between these there is no union but a species of joint peculiar to themselves; the hole in the middle thus formed appears unusually small.

In the specimen from which the above description is taken, there is also a fore foot, paddle, or fin,—for it is difficult to say which it should be called,—and which, though not quite perfect, is more so than in any other extant specimen. It presents nothing like the thumb or claw for laying hold, which distinguishes the animals that occasionally inhabit the sea, and come ashore to lay eggs or deposit young. If it be called a fin, it is to be understood as made up of bony materials, the joints of which are extremely numerous, so that it may possibly perform the same office.

An illustrative drawing accompanies this paper.

Some Experiments on the Fungi which constitute the Colouring Matter of the Red Snow discovered in Baffin's Bay. By Francis Bauer, *Esq. F.L.S. In a Letter addressed to the Right Hon. Sir* Joseph Banks, *Bart. G.C.B. P.R.S. &c. &c.* Read May 11, 1820. [*Phil. Trans.* 1820, *p.* 165.]

To ascertain whether the fungi mentioned in the title of this paper vegetate in the snow, Mr. Bauer put a small portion of them into a phial filled with compressed snow, and placed in the open air in a N.W. aspect. In fifty-two hours they had formed a red sediment; and the snow being thawed the water was poured off, and a fresh

portion added as before. In two days the mass of fungi was raised in little pyramids, which gradually increased in height, so as nearly to fill the phial, and occupied the cells of the mass of ice. A thaw now continued for some time, and the fungi fell to the bottom of the water in the phial, where they occupied about double their original bulk, having sustained an increase to that amount during their vegetation in the ice and snow.

In water these fungi appear also perceptible of vegetation, but they produce new fungi of a green instead of a red colour. By exposure to excessive cold the primitive fungi are killed, but their seed still retains vitality, and if immersed in snow regenerates new fungi, generally of a red colour. The author thinks that snow is undoubtedly the native soil of these fungi.

This paper is illustrated by a drawing, showing the original appearance of the fungi in the snow water from Baffin's Bay, and their gradual increase in the phials, as described in this abstract.

Some Account of the Dugong. By Sir Thomas Stamford Raffles, *Governor of Sumatra. Communicated in a Letter to Sir* Everard Home, *Bart. V.P.R.S.* Read May 18, 1820. [*Phil. Trans.* 1820, *p.* 174.]

The form of the Dugong resembles that of the common Cetacea. The skin is smooth and thick, with a few scattered hairs, and the head small in proportion, with two short tusks projecting from the extremity of the upper jaw. The place of the incisors is substituted by the rough bristly surfaces of the palate and jaws, which enable the animal to browse upon marine vegetables. There are twelve cylindrical molares, with flat crowns. The aperture of the ears is remarkably small. There are no dorsal or ventral fins; but the place of the anterior extremities is supplied by fins, which, however, are not capable of supporting the animal when out of water.

Upon dissection, the skin was found three quarters of an inch thick. The stomach has two appendages opening into it, near the junction of the duodenum; the intestinal canal is long; the liver has two large and two smaller lobes, one of which is tongue-shaped and covers the gall-bladder; the kidneys are large, and the urinary bladder probably capable of considerable distention; the testicles are placed a little below the kidneys; the urethra opens in a small tubercle between the two lobes of the glans penis.

In the thorax the thymus gland is large, black, and friable; the lungs not lobulated; and the ventricles of the heart, being separated at their points, give it a double appearance.

In regard to the skeleton, the head is remarkable for the manner in which the anterior part of the upper jaw bends downwards, the lower jaw being proportionally truncated. There are fifty-two vertebræ, eighteen ribs on each side, and the sternum is bifurcate at the apex, and articulated to the cartilages of the upper ribs. There is no pelvis nor posterior extremities, but opposite the eighth or tenth

lumbar vertebræ are two narrow flat bones, lodged in the flesh, one on each side. The scapula is thick, and the humerus, radius, and ulna, short and strong.

The flesh of this animal is delicate and juicy, resembling young beef. It is only found in shallows and inlets of the sea, and the greatest number is said to be taken during the northerly monsoon, near the mouth of the Johore river, in the inlet of the sea between Singapore island and the main; they seldom exceed eight or nine feet in length, though they probably grow much larger, but are then too strong to be caught.

Observations on the Human Urethra, showing its internal Structure, as it appeared in the Microscope of F. Bauer, *Esq. By Sir* Everard Home, *Bart. V.P.R.S.* Read June 1, 1820. [*Phil. Trans.* 1820, *p.* 183.]

Mr. Bauer has discovered, by the aid of the microscope, that the human urethra is made up of two parts, an internal membrane and an external muscular covering; the former, very thin and destitute of fibres, is thrown into folds in a collapsed state, and upon its surface are numerous orifices of glands; the latter is made up of short interwoven fibres, forming fasciculi united by an elastic substance of the consistence of mucus: these observations show the fallacy of the common opinion, that the lining of the urethra consists of circular contractile fibres, and throw a new light upon the disease called Stricture; a spasmodic stricture being a contraction of a small portion of the longitudinal muscular fibres, while the others are relaxed; and a permanent stricture consisting in the exudation of coagulable lymph, in consequence of inflammation, between the fasciculi of muscular fibres and upon the internal membrane.

After adverting to what is known respecting the structure of the corpus spongiosum, and corpora cavernosa, the author proceeds to state the result of Mr. Bauer's examination of those parts. The cellular structure of the corpora cavernosa is made up of many thin membranous plates, very elastic, and so connected as to form a trellis-work, the edge of which is attached to the elastic ligamentous substance which surrounds them, and which forms the septum that separates them. The structure of the corpus spongiosum resembles that of the corpora cavernosa, except that the parts are formed upon a smaller scale, and that there are no muscular fibres in its ligamentous elastic covering. The various details and descriptions in this paper are illustrated by Mr. Bauer's drawings.

On the Errors in Longitude as determined by Chronometers at Sea, arising from the Action of the Iron in the Ships upon the Chronometers. By George Fisher, *Esq. Communicated by* John Barrow, *Esq. F.R.S.* Read June 8, 1820. [*Phil. Trans.* 1820, *p.* 196.]

The sudden alterations in the rates of chronometers, when taken on board ships, are generally ascribed to the motion of the vessel; but

from circumstances connected with the chronometers of the Dorothea and Trent, during the late voyage to the North Pole, the author is induced to refer these alterations to other causes: he found that in all cases the gaining rates were increased and their losing ones diminished on ship-board. That this acceleration does not arise from the ship's motion, was shown by its occurrence when the Dorothea and Trent were beset with ice, and when they were at anchor close in shore without any perceptible motion; nor does it appear that change of temperature was at any time the cause of this change of rates. That the iron in ships becomes magnetic is shown by its polarity, the whole forming, as it were, a large magnet, having its south pole on deck and its north pole below. The inner rim of the balance of chronometers, which is made of steel, will, therefore, be liable to magnetic action, which will be sufficient to cause a very sensible alteration in their rate of going.

Mr. Fisher concludes this communication with some account of experiments on the action of magnets upon chronometers placed in various positions with respect to their balances, by which it appears that an acceleration in these cases always ensues. It also appears probable, he observes, that the force of the balance spring is affected by the same cause, since chronometers in which they are made of gold, though more difficult to adjust, keep better rates at sea than others.

An appendix, containing tables of rates furnished by Mr. Coleman, is annexed to this paper.

An Account of a New Mode of performing the High Operation for the Stone. By Sir Everard Home, *Bart. V.P.R.S.* Read June 15, 1820. [*Phil. Trans.* 1820, *p.* 209.]

The method of removing a stone from the bladder, which is detailed in this communication, is as follows:—An incision is made in the direction of the linea alba, beginning at the pubes, and four inches long, down to the tendon. The linea alba is then pierced close to the pubes, and divided to the extent of three inches. A silver catheter is now passed into the bladder; and its point being felt through the wound in the fundus of the bladder, a stilet is forced through it, followed by the end of the catheter. The stilet is then withdrawn, and the opening through the fundus of the bladder enlarged towards the pubes by a probe-pointed bistoury. The catheter is then withdrawn, and the stone removed. A flexible gum catheter is lastly introduced, by which the urine passes off. The subject of the above operation was a lad sixteen years of age. In ten days the wound of the bladder healed, and he made water freely by the urethra. The stone was very rough, and about an ounce in weight.

A Sketch of an Analysis and Notation applicable to the Estimation of the Value of Life Contingencies. *By* Benjamin Gompertz, *Esq.* *F.R.S.* Read June 29, 1820. [*Phil. Trans.* 1820, *p.* 214.]

Mr. Gompertz begins by establishing a system of notation intended to avoid unnecessary repetitions and circumlocution, and proceeds to apply his abbreviated expressions to a more accurate determination of the value of a number of joint lives, according to any given tables of mortality, than can be obtained by the common approximations. He afterwards investigates the probabilities of the survivorship of two persons of different ages, who were known only to be living at one time and dead at another, which, within certain limits, are supposed to be equal: and he inquires into the conditions of mortality that would be requisite in order that this proposition should be accurately true. He then applies his method to the problems which have been solved by Mr. Morgan in the Philosophical Transactions, and copied by Mr. Baily in his work on Assurances, relating to some complicated contingencies and survivorships on different suppositions respecting the decrement of life, employing various integrations, summations, and approximations in this elaborate inquiry, but without obtaining any results which it is possible to specify in an abstract, as giving a general idea of the nature of his improvements, without entering into forms of expression directed almost as much to the eye as to the understanding.

On the Measurement of Snowdon, by the Thermometrical Barometer. *By the Rev.* F. J. H. Wollaston, *B.D. F.R.S.* Read June 29, 1820. [*Phil. Trans.* 1820, *p.* 295.]

After adverting to the statements contained in his former paper upon the use of the above-mentioned instrument, and giving certain tables requisite for determining the necessary corrections arising out of the want of uniformity in the variations of the boiling temperature of water at certain elevations, the author details the means which he resorted to for estimating the height of Snowdon by this instrument, and compares his results with the trigonometrical and barometrical measurement of General Roy.

The height, as obtained by the thermometric barometer, properly corrected from the north end of Carnarvon Quay to the summit, is 3546·25 feet. General Roy's trigonometrical measurement gives 3555·4 feet, and barometrically 3548·9 feet. During this visit to Carnarvon, the author also took the opportunity of ascertaining, by the same means, the height of Moel Elio, also measured by General Roy. He makes it 2350·55 feet, while General Roy's trigonometrical measurement gives 2371 feet, and the barometer 2391·8 feet. This discordance, the author thinks, may be referred to the indeterminate form of Moel Elio rendering the point of observation less definite.

Archdeacon Wollaston concludes this paper with a description of some improvements in the construction of this instrument.

On Sounds Inaudible by certain Ears. *By* William Hyde Wollaston, *M.D. P.R.S.* Read June 29, 1820. [*Phil. Trans.* 1820, *p.* 306.]

In this communication the author describes a peculiar insensibility to certain sounds in the ears of persons not otherwise deaf, which he was led to observe by trying different modes of lessening the sense of hearing in himself; when he found, that by closing the nose and mouth, and expanding the chest, the membrana tympani, thrown into a state of tension by external pressure, made the ear insensible to grave tones, without affecting the perception of sharper sounds. In this case the ear was insensible to all sounds below F marked by the bass cliff.

In the natural healthy state of the ear, there seems to be no limit to the power of discerning low sounds; but if we attend to the opposite extremity of the scale of audible sounds, and with a series of pipes, exceeding each other in sharpness, examine their effects successively upon the ears of different persons, we shall find considerable difference in their powers of hearing them, and see reason to infer that human hearing is more confined than has been supposed. Dr. Wollaston's attention was called to this circumstance by finding a person insensible to the sound of a small organ pipe, which, with respect to acuteness, was far within the limits of his own hearing. By subsequent examination, this person's hearing was found to terminate at a note four octaves above the middle E of the pianoforte. Other cases of the insensibility of the ear of certain persons to high sounds are next adverted to; such as to the chirping of the grasshopper, the cricket, the sparrow, and the bat; the latter being about five octaves above the middle E of the piano. The limit of the author's own sense of hearing is at about six octaves above the middle E; and, from numerous trials, he is induced to think that, at the limit of hearing, the interval of a single note between two sounds may be sufficient to render the higher note inaudible, although the lower one is heard distinctly.

The range of human hearing includes more than nine octaves, the whole of which are distinct to most ears, though the vibrations of a note at the higher extreme are 600 or 700 times more frequent than those which constitute the gravest audible sound; and as vibrations incomparably more frequent may exist, we may imagine, says the author, that animals like the Grylli, whose powers appear to commence nearly where ours terminate, may hear still sharper sounds, which we do not know to exist; and that there may be insects hearing nothing in common with us, but endued with a power of exciting, and a sense that perceives the same vibrations which constitute our ordinary sounds, but so remote that the animal who perceives them may be said to possess another sense, agreeing with our own, solely in the medium by which it is excited, and possibly wholly unaffected by those slower vibrations of which we are sensible.

Particulars respecting the Anatomy of the Dugong, intended as a Supplement to Sir T. S. Raffles's *Account of that Animal. By Sir* Everard Home, *Bart. F.R.S.* Read June 29, 1820. [*Phil. Trans.* 1820, *p.* 315.]

The object of this communication is to complete the anatomical description of the Dugong, already presented to the Society by Sir Thomas Stamford Raffles, who has sent the author a young female animal entire, together with the viscera and skeleton of a male. Drawings representing the external form of the animal, and of its several parts, are annexed. Sir Everard particularly describes the peculiar structure of the stomach of this animal, which differs from all others, and is so complex that description is scarcely intelligible without the aid of a drawing. In some respects it resembles that of the whale, the peccari, hippopotamus, and beaver; at least it contains parts met with in the stomach of those tribes, but the parts are differently situated.

On the Compressibility of Water. By Jacob Perkins, *Esq. Communicated by the late Right Hon. Sir* Joseph Banks, *Bart. G.C.B. P.R.S.* Read June 29, 1820. [*Phil. Trans.* 1820, *p.* 324.]

A hollow and water-tight cylinder, 3 inches in diameter and 18 inches long, with a rod five sixteenths of an inch diameter, sliding through a stuffing box at one of its extremities, and having upon it a flexible ring, placed just above the stuffing box, was filled with water, and put into a cannon of sufficient dimensions, fixed vertically in the earth, with its touch-hole plugged, and its muzzle about 18 inches above ground. A strong cap was firmly secured upon the mouth of the cannon, with a small forcing pump tightly screwed into its centre. There was an aperture secured by a valve, one pound pressure upon which indicated an atmosphere. Upon forcing water into the cannon, it was found that when the instrument contained within it, called by the author a Piezometer, had suffered a pressure equal to a hundred atmospheres, the position of the ring upon the piston indicated that it had been forced into the cylinder to a depth of eight inches, showing that the water had suffered a compression of about 1 per cent.; the same effect was produced by sinking the piezometer to a depth of 500 fathoms in the ocean. Upon sinking a strong empty bottle, well corked and tied down, to a depth of 300 fathoms, the neck only was found remaining upon the line; from the appearance of which it was evident, that a quantity of water, sufficient to fill the bottle, had at that depth been forced through the cork and its coverings, and that the water expanding during the drawing up of the bottle had broken it.

It appearing to the author that the original indication of the piezometer was rendered erroneous by the collapsing of the leather upon the rod under such great pressure, he employed a modification of the instrument, in which a valve was used as a substitute for the piston,

the cylinder being flattened so as to yield to the expansion of the water forced in. By weighing this instrument full of water, before and after it had been submitted to the pressure, the quantity of water forced in was ascertained; whence it appeared that under a pressure of 326 atmospheres, the water had sustained an increase of $3\frac{1}{2}$ per cent. In a future communication the author proposes to detail the results of a new set of experiments on the compressibility of water, which he hopes will be susceptible of greater precision.

Astronomical Observations. By Stephen Groombridge, *Esq. F.R.S.* Read June 29, 1820. [*Phil. Trans.* 1820, *p.* 330.]

In the present improved state of astronomical observations, it is material to possess the readiest and most accurate means of finding the apparent time. The right ascension of certain fixed stars having been precisely obtained relatively with each other, and with the equinoctial points during the course of many years, affords the direct method of ascertaining the right ascension of the mid-heaven: hence the convenience of having the corrections of these stars in the form of tables, that the same may be taken out at one view with the arguments of the sun's longitude, and of the moon's node. For this purpose the mean diurnal motion is adapted to the longitude of the sun, as found in the Nautical Almanac, at the time the star passes the meridian. The mean epoch is reduced to the vernal equinox less four seconds, in order to render the corrections additive; which, being an universal period, the same applies to all parts of the world. To these tables Mr. Groombridge has subjoined some observations of the planets at and near the oppositions; also of the solstices of the last two years, and of the comet of 1819.

On the Black Rete Mucosum of the Negro, being a Defence against the Scorching Effect of the Sun's Rays. By Sir Everard Home, *Bart. F.R.S.* Read November 9, 1820. [*Phil. Trans.* 1821, *p.* 1.]

The use of the black rete mucosum of the negro is a subject which has fruitlessly engaged the attention of the physiologist. The author's mind was directed to this inquiry by the circumstance of a silver fish having its back scorched in consequence of the removal of some trees which shaded the pond in which it lived; this recalled to Sir Everard's recollection the circumstance of his having suffered severely from the scorching sun of the tropic, upon parts of the body protected from the direct rays of the sun by thin white linen, and led him to suspect that the noxious effects were derived not, as has commonly been supposed, from the mere heating power of the sun's rays, but from the joint agency of heat and light: he therefore made certain experiments, detailed in this paper, which show that the face and hands may be exposed to a temperature of 100° to 120°, without producing pain, provided light be excluded; but that if the same, or even an inferior degree of heat, be produced by the direct light of the sun, it

scorches and elevates the cuticle in blisters; this effect is prevented by covering the hand or face with thin black kerseymere, and the same purpose is attained by the black rete mucosum of the negro. In these cases perspiration comes on, which it does not in the former, and the surface, though actually hotter, is uninjured.

Nature has provided a similar defence from the injury of light, by placing a black pigment at the bottom of the eye of those animals who are exposed to the intensity of the sun's rays; whereas in the inhabitants of shady and dark situations the pigment is either pale, or altogether wanting.

The author concludes this paper by ascribing the beneficial effects which he has shown to result from the black matter in the negro's skin, and at the bottom of the eyes in certain animals, to the power which black surfaces possess of converting the radiant matter of the sun into sensible heat.

On the Magnetic Phenomena produced by Electricity. In a Letter from Sir Humphry Davy, *Bart. F.R.S. to* W. H. Wollaston, *M.D. P.R.S.* Read November 16, 1820. [*Phil. Trans.* 1821, *p.* 7.]

In repeating the experiments of Oersted, Sir Humphry found that with a voltaic battery of 100 pair of 4-inch plates, the south pole of a magnetic needle placed under the communicating wire of platinum (the positive end of the apparatus being on the right) was strongly attracted by the wire, which was shown to be itself magnetic, by its power of attracting steel filings, and of communicating permanent magnetism to steel bars attached to it transversely, while similar bars placed parallel to the wire were only magnetic during its connection with the voltaic apparatus. The actual contact, however, of the steel wire with that of platinum, or other metal forming the voltaic conductor, is not necessary, for magnetism was communicated to a needle placed transversely to it, but at some distance.

Sir Humphry Davy next details some experiments, showing that the magnetic power is proportionate to the quantity of electricity passing through a given space, without any relation to the transmitting metal; and that the finer the wires, the stronger their magnetism. He found an analogous effect produced by the discharge of a Leyden phial through a wire; and by passing the discharge of a Leyden battery of 17 square feet through a silver wire, with a steel bar transversely attached to it of two inches in length, the latter became powerfully and permanently magnetic. The same effect was produced at a distance of five inches through air, water, and even through thick plates of glass.

When several wires parallel to each other form part of the same circuit, each becomes similarly magnetic to the single wire; and the opposite sides of such wires are in different magnetic states, and consequently attract each other. By arranging two voltaic batteries parallel to each other, the positive end of the one being opposite to the negative end of the other, and transmitting their electricities

through two wires, such wires repel each other, because their opposite sides are in similar magnetic states.

A Communication of a singular Fact in Natural History. By the Right Honourable the Earl of Morton, *F.R.S. In a Letter addressed to the President.* Read November 23, 1820. [*Phil. Trans.* 1821, *p.* 20.]

Being desirous of domesticating the Quagga in this country, his Lordship endeavoured to procure some individuals of that species, but being disappointed in obtaining a female, an attempt was made to breed from the male and an Arabian chestnut mare; the result was a female hybrid, now five years old, and showing her mixed origin both in form and colour.

The Arabian mare has since been bred from, by a black Arabian horse, and the produce, namely, a two year old filly and a year old colt, though in most respects fine specimens of the Arabian breed, are marked with certain stripes and lines belonging exclusively to the Quagga: the manes are especially unlike those of the Arabian breed. It is a striking fact, observes his Lordship, that so many features not belonging to the dam, should in two successive instances be transferred by her to the progeny of a sire who has them not.

Particulars of a Fact, nearly similar to that related by Lord Morton, *communicated to the President, in a Letter from* Daniel Giles, *Esq.* Read November 23, 1820. [*Phil. Trans.* 1821, *p.* 23.]

In the litter of a black and white sow, by a boar of the wild breed, the chestnut colour of the boar strongly prevailed; a second litter from the same mother, by a boar of a very different breed, retained many peculiarities of the wild breed; and even in a third litter the chestnut colour was to a certain extent evident.

The Croonian Lecture. Microscopical Observations on the following Subjects. On the Brain and Nerves; showing that the Materials of which they are composed exist in the Blood. On the Discovery of Valves in the Branches of the Vas breve, lying between the Villous and Muscular Coats of the Stomach. On the Structure of the Spleen. By Sir Everard Home, *Bart. V.P.R.S.* Read December 7, 1820. [*Phil. Trans.* 1821, *p.* 25.]

By a microscopic examination of the retina and optic nerve, Mr. Bauer found them to consist of globules of $\frac{1}{2000}$ to $\frac{1}{4000}$ of an inch diameter, united by a transparent viscid and coagulable gelatinous fluid: the brain also, according to the same observer, consists of the same globules, united by the viscid jelly, and forming a fibrous arrangement.

After describing the peculiarities in texture of the different parts of the brain, Sir Everard adverts to the circumstance of lymphatics

never having been found in that organ, and shows that the veins are supplied with valves, and perform the office of absorbents, carrying their contents into the superior longitudinal sinus, which appears rather to be a reservoir than a vein, for the fluid that passes through it is not simply circulating blood, but contains the colouring matter in a decomposed state, and is black as ink.

There can be little doubt, says Sir Everard, that the communication of sensation and volition is more or less dependent upon the viscid mucus which links the globules of the brain and nerves together; he then proceeds to show its existence in the blood, and that it is the medium by which the colouring matter is attached to the surface of the red globules. It would appear therefore, continues the author, that the principal materials of which the body is composed are found in the blood, with the exception of fat; fat, however, is found in the blood of the skate and salmon, and perhaps is united with the alkali in human blood.

Sir Everard next details the result of his examination of the veins of the coats of the stomach, and of the vasa brevia, which are also supplied with valves, and which act the part of absorbents. In tracing these veins towards the cavity of the stomach, they became indistinct just as they entered the villi.

This paper concludes with some observations respecting the structure and uses of the spleen, from which Sir Everard concludes that it is a reservoir to receive the superabundant serum carried into the circulation from the stomach into the splenic vein; and not only of the serum, but of the coagulable lymph, globules, soluble mucus, and colouring matter, which are carried to the thoracic duct when wanted.

On Two New Compounds of Chlorine and Carbon, and on a New Compound of Iodine, Carbon, and Hydrogen. By Mr. Faraday, *Chemical Assistant in the Royal Institution. Communicated by* W. T. Brande, *Esq. Sec. R.S. and Prof. Chem. R.I.* Read December 21, 1820. [*Phil. Trans.* 1821, *p.* 47.]

After some general observations respecting the action of chlorine upon compounds containing carbon, and more especially upon carburetted hydrogen gas, Mr. Faraday details the processes by which he succeeded in obtaining two binary compounds of carbon and chlorine; the first, which he calls perchloride of carbon, was formed by exposing the triple compound of carbon, hydrogen, and chlorine, with excess of chlorine, to the agency of the direct solar rays; muriatic acid was formed, and a white crystalline compound at the same time generated. The author next describes the method of purifying this compound, and details its properties, which are briefly these:—it forms crystals, which appear to result from a primitive octahedron; it does not conduct electricity; it is slowly volatile, like camphor, at common temperatures, fusing at 320°, and boiling at 360°. It is not easily combustible; but when retained in the flame of the lamp,

produces a red flame, with the formation of muriatic acid; it is insoluble in water, and readily soluble in alcohol, ether, and oils; and nearly insoluble in acids. When heated with several metallic peroxides it is decomposed with the production of carbonic acid, and a metallic perchloride.

The author describes several experiments made with a view to ascertain the proportions in which the carbon and chlorine exist in this compound, from which it appears, that as one volume of olefiant gas requires five volumes of chlorine for its conversion into muriatic acid and this new chloride, and as four volumes of muriatic acid are formed, so three volumes of chlorine must unite to two of carbon to form the solid chloride.

When this perchloride is passed through a red hot tube chlorine is evolved, and a liquid compound of carbon and chlorine is obtained, which assumes the form of a vapour at 170°, and which, like the former, is insoluble in water, but soluble in alcohol and ether, and burns with the same phenomena as the solid chloride.

The results of the author's analytical experiments upon this fluid compound, induce him to regard it as consisting of one proportion of each of its elements.

By exposing the vapour of iodine and olefiant gas to the sun's rays, Mr. Faraday obtained a colourless crystalline compound, difficultly combustible, but decomposable at a high temperature, of a sweet taste and aromatic odour, and composed of iodine, carbon, and hydrogen. He has not yet succeeded in forming a binary compound of carbon and iodine, though his experiments leave little doubt of the existence of such a compound, and of the possibility of forming it when aided by a bright sunshine.

An Account of the Comparison of various British Standards of Linear Measure. By Captain Henry Kater, *F.R.S. &c.* Read January 18, 1821. [*Phil. Trans.* 1821, *p.* 75.]

The Commissioners of Weights and Measures having recommended, for the legal determination of the standard yard, that employed by General Roy in the measurement of a base on Hounslow Heath, as a foundation for the trigonometrical operations that have been carried on by the Ordnance throughout the country, it became necessary to examine the standard to which the report alludes, with the intention of subsequently deriving from it a scale of feet and inches.

This standard consists of an iron bar, 20 feet long, described by Captain Kater, in which gold points are inserted, at the distance of 40 inches from each other, from a standard scale of Mr. Ramsden's, which was declared similar to that of General Roy, and also to that of the Royal Society; but on examining these scales, Captain Kater ascertained the existence of material differences between them; and being aware of the existence of other standards of high authority, he procured and compared them.

The author then details at length the methods by which he effected these comparisons, and gives tables of the various measurements, which are afterwards comprised in the following abstract, taking Colonel Lambton's standard, used in the survey of India, as the point of departure, in consequence of its being the shortest. Captain Kater finds the excess on 36 inches to be as follows:—

Sir George Shuckburgh's standard	+ 000642
Bird's standard, 1760	+ 000659
General Roy's scale	+ 001537
Royal Society's standard	+ 002007
Ramsden's bar, used in the trigonometrical survey	+ 003147

The author then proceeds to investigate the effect of these differences on the figure of the earth, and arrives at the conclusion, that the comparison given in the abstract of Colonel Lambton's paper, in the Philosophical Transactions for 1818, namely, [illegible] should be [illegible], which agrees very nearly with the deduction of M. Laplace from the lunar irregularities; with the result of Dr. Young's investigation, by a comparison of the mean with the superficial density of the earth; and with the conjecture hazarded by the author, from the compression given by the experiments on the length of the pendulum at Unst and Portsoy.

An Account of the Urinary Organs and Urine of Two Species of the genus Rana. By John Davy, *M.D. F.R.S.* Read January 18, 1821. [*Phil. Trans.* 1821, *p.* 95.]

The species of the genus Rana, adverted to in the title of this paper, are the *Rana taurina*, or bull frog, and the *Bufo fuscus*, or brown toad.

The kidneys of the bull frog are lobulated, and the ureters terminate in the rectum, between the orifice of the bladder and the anus; the bladder is large, and its orifice well calculated to receive the urine as it flows from the ureters, its escape from the rectum being prevented by the sphincter muscle of the anus. In the brown toad the ureters have an analogous termination; but the bladder when distended resembles two oval bags, freely communicating just over the symphysis pubis, to which they are firmly attached.

The urine of the bull frog is without action on vegetable colours, and contains urea, with traces of sea salt and a little phosphate of lime.

The author concludes this paper with some remarks relative to the dissimilarity of the urine in animals, whose diet is similar, and considers the nature of that secretion as depending rather upon the intimate and invisible structure of the kidney than upon the kind of food which they consume. The brown toad and the lizard both live upon flies, but their urine is very different; the parrot eats vegetables only, and the snake feeds exclusively upon animal matter, yet in them the urinary secretion is in all main points alike, uric acid being the predominant ingredient in both.

An Account of a Micrometer made of Rock Crystal. By G. Dollond, *F.R.S.* Read January 25, 1821. [*Phil. Trans.* 1821, *p.* 101.]

The author's object in this communication is to describe a more simple application of rock crystal to the purposes of micrometrical measurements in telescopes than any hitherto adopted. His improvement consists in the substitution of a sphere of rock crystal, in place of the usual eye-glass, by which the trouble of angular cutting is done away, it being only necessary to form the lens of a proper diameter for the focal length required.

Another advantage obtained by Mr. Dollond's micrometer is that of being able to take the angle on each side zero without reversing the eye tube; and also of taking intermediate angles, by moving the axis in which the sphere is placed; and, thirdly, it possesses the property of an eye tube not intended for micrometrical measurement, for when the axis of the crystal is parallel to that of the object glass, only one image is formed, and that perfectly distinct. After adverting to some other advantages resulting from this improvement, Mr. Dollond proceeds more particularly to describe the contrivance by reference to an annexed drawing.

The Bakerian Lecture. On the best kind of Steel and Form for a Compass Needle. By Captain Henry Kater, *F.R.S.* Read Feb. 1. 1821. [*Phil. Trans.* 1821, *p.* 104.]

On the return of the first expedition from the discovery of a North-west Passage, the compasses were reported to have become nearly useless, from the diminution of the directive force consequent upon the near approach to the magnetic pole. The azimuth compasses on that occasion being of the author's invention, he was anxious that the second expedition should be furnished with instruments combining the utmost power and sensibility; and was consequently led to the researches, the mode of conducting which, with their results, form the subject of this lecture.

In respect to the best material for the construction of compass needles, Captain Kater found that clock springs made of sheer steel were capable of receiving the greatest magnetic force, and that in forming the needle it should be exposed as little as possible to heat, by which its capability of receiving magnetism is diminished.

The form best adapted for the needle is the pierced rhombus, of about five inches long and two wide, and it should be tempered by previous hardening at a red heat, and then softened from the middle to about an inch from each extremity, by due exposure to heat, so as to dissipate the blue colour. The polish of the needle appears to have no effect upon its magnetism; but in the same plate of steel, of the size of a few square inches only, portions were found varying considerably in their power of receiving magnetism, though not apparently differing in other respects.

The best mode of communicating magnetism to a needle appears.

from Captain Kater's experiments, to consist in placing it in the magnetic meridian, joining the opposite poles of a pair of bar magnets, (they being in the same line,) and laying them flat upon the needle, with their poles upon its centre; then, having elevated the distant extremities of the magnets, so that they may form an angle of about 2° or 3° with the needle, they are to be drawn from the centre to its extremities, carefully preserving the same inclination; and having joined the poles of the magnets at a distance from the needle, the operation is to be repeated ten or twelve times upon each surface.

In needles from five to eight inches long, their weights being equal, Captain Kater found their directive forces nearly as the lengths; but in needles of nearly the same length and form, the directive force is as the mass, and not dependent upon the extent of surface.

Lastly, the author ascertained that the deviation of a compass needle, occasioned by the attraction of soft iron, depends, as Mr. Barlow has advanced, upon extent of surface, and is wholly independent of the mass; excepting that a thickness of the iron, amounting to about two tenths of an inch, is requisite to the complete development of the attractive energy.

Notice respecting a Volcanic Appearance in the Moon. In a Letter addressed to the President. By Captain Henry Kater, *F.R.S.* Read February 8, 1821. [*Phil. Trans.* 1821, *p.* 130.]

This volcano was first observed by Captain Kater on Sunday the 4th of February, the moon being then two days old, with a Newtonian telescope of 6¼ inches aperture, and a power of 74. The position of the volcano is shown in an annexed drawing; its appearance was that of a small nebula, subtending an angle of 3 or 4 seconds, and its brightness very variable.

It was again observed on the evenings of the 5th and 6th, but was then more faint, though occasionally exhibiting the appearance of a luminous point, like a star of the 6th or 7th magnitude. Captain Kater thinks that the distance of the volcano from the edge of the moon was about one tenth of her diameter; and the angle which it formed with a line joining the cusps on the last-mentioned evening, was about 50°. On the 7th it was scarcely visible, in consequence, probably, of the increased light of the moon.

A Further Account of Fossil Bones discovered in Caverns inclosed in the Lime-stone Rocks at Plymouth. By Joseph Whidbey, *Esq. In a Letter addressed to Sir* Everard Home, *Bart. V.P.R.S.* Read February 8, 1821. [*Phil. Trans.* 1821, *p.* 133.]

These bones were found not far from those previously described by Mr. Whidbey, and in a similar situation; the cavern being entirely inclosed in the surrounding rock, and without the smallest appearance of any communication ever having existed with the sur-

face. There were no staluctites in this cavern, as there generally is in those which contain no bones, and it was perfectly dry and free from rubbish.

From a note annexed to this letter by Sir Everard Home, the bones alluded to appear to be the grinder of the upper jaw of the single-horned rhinoceros. Two grinders, two tusks, and portions of two tibiæ of the brown or black bear; and portions of bones of an animal of the deer kind.

These specimens are deposited in the Museum of the College of Surgeons.

On the Aëriform Compounds of Charcoal and Hydrogen; with an Account of some Additional Experiments on the Gases from Oil and from Coal. By Willian Henry, *M.D. F.R.S. &c.* Read February 22, 1821. [*Phil. Trans.* 1821, *p.* 136.]

In this paper, after adverting to the sources, properties, and composition of carburetted hydrogen obtained from stagnant water, and of olefiant gas procured from the decomposition of alcohol; and after examining the agency of chlorine upon these compounds; the author proceeds to examine the gas procured by the decomposition of oil and of coal at high temperatures. The former, or oil gas, is shown to vary considerably in composition and properties, according to the temperature at which it is procured; and though no temperature short of ignition is sufficient for the decomposition of oil into permanent combustible gases, yet the lower the heat the more combustible is the gas, and better suited to artificial illumination.

In analysing these gases, Dr. Henry always found them mixtures of olefiant, carburetted hydrogen, hydrogen, and carbonic oxide gases. Dr. Henry separated the first by the action of chlorine, and from the detonation of the residue with oxygen, as compared with an artificial mixture of known composition, he ascertained the relative proportions of its components.

It appears from the tables exhibiting these results, that in oil gas the proportion of carbonic oxide is greater than in that from coal, but that carburetted hydrogen is most abundant in the latter. The proportion of hydrogen appears to increase in both as they are formed at higher temperatures, and is always greatest in the latter portions of coal gas; but Dr. Henry never found that either oil or coal gas, after the action of chlorine with the exclusion of light, presented a residue of pure hydrogen.

In the concluding section of this paper, the author details some experiments which led him to consider that portion of oil gas which is condensible by chlorine, not as mere olefiant gas, but as a peculiar compound, requiring nearly two volumes of oxygen more for its combustion than an equal quantity of olefiant gas, and affording one additional volume of carbonic acid; he therefore thinks that it must be considered either as containing a new compound of carbon and

hydrogen, or as deriving its peculiarities from an inflammable vapour. To this new gaseous compound much of the illuminating power of coal and oil gas is to be attributed.

An Account of Experiments to determine the Acceleration of the Pendulum in different Latitudes. By Captain Edward Sabine, *of the Royal Regiment of Artillery, F.R.S. and F.L.S.* Read March 8, 1821. [*Phil. Trans.* 1821, *p.* 163.]

The clocks and pendulums used in these experiments are the property of the Royal Society, and were prepared by their direction, under the superintendance of Captain Kater, whose description of them is quoted by the author at the commencement of this paper.

The experiments were made during two voyages of discovery in search of a North-west Passage, the first in 1818, and the second in 1819 and 1820; and Captain Sabine details in succession the proceedings at each station, where an opportunity was afforded of landing and setting up the clocks; and concludes by recapitulating the number of vibrations made by each pendulum in the different latitudes in which it was tried, and by stating the deductions regarding the figure of the earth which follow from the acceleration thus determined. In the first voyage, the number of vibrations was ascertained at two stations only; namely, at Gardie House on the Island of Brassa, and on Waygat, or Hare Island, on the West coast of Greenland; the latitude of the first being 60° 9′ 42″ N., and of the second 70° 26′ 17″ N. The number of vibrations in a mean solar day at London being 86497·4, at Brassa they were 86530·507, and at Hare Island 86562·6386; giving an acceleration of 33·107 vibrations between London and Brassa, and of 32·1316 between Brassa and Hare Island; or 65·2386 between London and Hare Island.

Captain Sabine next proceeds to detail the preliminary experiments relating to the pendulums, and the results of his various observations, made during the second voyage; from which it appears that at Melville Island in the Polar sea, in latitude 74° 47′ 12·4″ N., the mean diurnal acceleration amounted to 74·734 vibrations. From the observations detailed at length in this paper, respecting the length of the seconds' pendulum, at the several places of observation, it appears that its length at London being, as ascertained by Captain Kater, 39·13929 inches, at Brassa it is 39·16929 inches; at Hare Island 39·1984, and at Melville Island 39·207 inches. This paper concludes with a table, showing the diminution of gravity from the pole to the equator, and the resulting ellipticity of the earth, deduced from the preceding observations. The method followed in obtaining these deductions is the same which is described by Captain Kater in the Philosophical Transactions for 1819.

Some Observations and Experiments on the Papyri found in the Ruins of Herculaneum. *By Sir* Humphry Davy, *Bart. P.R.S.* Read March 15, 1821. [*Phil. Trans.* 1821, *p.* 191.]

Having in some preliminary experiments upon fragments of a roll of papyrus found at Herculaneum, the leaves of which adhered very strongly together, ascertained that it afforded, by exposure to heat, a considerable quantity of inflammable gaseous matter; that when digested in nitric and muriatic ether it coloured those fluids; and that, when immersed in an atmosphere of chlorine, it was evidently acted upon;—Sir Humphry Davy concluded that there yet remained in these papyri no inconsiderable portion of undecomposed vegetable matter. With this information he proceeded to examine the collection preserved at Naples; and after detailing the state of the manuscripts, he describes the methods resorted to for detaching the layers and ascertaining their contents. These methods were considerably varied, according to the state of the manuscripts. Those which were pale chestnut-coloured, and covered with white ashes, were so far destroyed as to render all attempts at arriving at any knowledge of their contents quite hopeless. Two manuscripts of firmer texture, and having the appearance of peat, were unrolled when acted upon by chlorine and heat, but they had been rendered illegible by the previous operation of water. All the best specimens of the black and more perfect manuscripts had been operated upon before Sir Humphry's arrival at Naples; so that of these there were only some remaining fragments, from many of which he however succeeded in obtaining parts of columns, by which their contents may be judged of. On the black manuscripts containing white earthy matter in their folds several experiments were tried, but they were rarely successful, from the firm agglutination of the fibres of the papyrus.

After having adverted to the various impediments that were opposed to these proceedings, by the persons under whose care and superintendence the Herculaneum manuscripts at Naples were placed, and which rendered it improper to attempt continuing the inquiry, Sir Humphry concludes this paper with some general observations. The Roman manuscripts are generally on a thicker papyrus, and in larger characters, though less perfect than the Greek ones. None of them contain those vestiges of oxide of iron which would have been detected had ink of galls and iron been employed: indeed it is probable, says Sir Humphry Davy, that the use of this ink and that of parchment were introduced at the same time; for the ink composed of charcoal and solution of glue will scarcely adhere to skin, whereas the free acid of the chemical ink partly dissolves the gelatine of the manuscripts, and the whole adheres as a mordant.

In the whole collection of manuscripts, no fragments of Greek, and only very few of Latin poetry, have been found; they are almost exclusively the productions of the Greek Epicurean philosophers and sophists.

Copies of the fragments unrolled by Sir Humphry Davy,—being fac-similes of the original imitations, executed by Sir William Gell, —are annexed to this communication.

Observations on Naphthaline, a peculiar Substance resembling a Concrete Essential Oil, which is apparently produced during the Decomposition of Coal Tar, by Exposure to a Red Heat. *By* J. Kidd, *M.D. Professor of Chemistry, Oxford. Communicated by* W. H. Wollaston, *M.D. F.R.S.* Read March 8, 1821. [*Phil. Trans.* 1821, *p.* 209.]

By passing coal tar through a red-hot iron tube, a portion of an aqueous fluid, and of a substance like tar, was obtained; the latter is black, soluble in ether, and partially in alcohol, of an aromatic odour, and sweetish taste. It was submitted to slow distillation, and among other products afforded naphthaline, a white concrete substance of an aromatic odour and taste, fusible at 180°, and scarcely soluble in water, but readily so in ether, alcohol, and oils.

Of the various characters of this substance, detailed by the author, its tendency to crystallize appears the most remarkable; its vapour condenses in rhombic plates, which are sometimes modified into hexagonal plates, by the incomplete development of the smaller angles of the usual rhomb.

The other substances obtained along with the naphthaline were ammoniacal water, and an oil of a bituminous and aromatic odour, boiling at 210°, and not congealing at 32°, highly inflammable, and readily soluble in alcohol and ether: there was also produced, during the latter part of the distillation, a yellow farina precipitable from its alcoholic solution by water, and fusible. Of these four substances, resulting from the distillation of the black liquid obtained by distilling coal tar, Dr. Kidd thinks that the water and the farina are products, and the other mere educts of the operation.

On the Aberrations of Compound Lenses and Object-Glasses. *By* J. F. W. Herschel, *Esq. F.R.S. &c.* Read March 22, 1821. [*Phil. Trans.* 1821, *p.* 222.]

To those mathematicians who have investigated the theory of the refracting telescope, it has often, says Mr. Herschel, been objected, that little practical benefit has resulted from their speculations. Although the simplest considerations suffice for correcting that part of the aberration which arises from the different refrangibility of the different coloured rays, yet in the more difficult part of the theory of optical instruments which relates to the correction of the spherical aberration, the necessity of algebraic investigation has always been acknowledged; although, however, the subject is confessedly within its reach, a variety of causes have interfered with its successful prosecution, and the best artists are content to work their glasses by empirical rules. In the investigations detailed in this paper, the

author's object is, first to present, under a general and uniform analysis, the whole theory of the aberration of spherical surfaces; and then to furnish practical results of easy computation to the artist, and applicable, by the simplest interpolations, to the ordinary materials on which he works. In pursuing these ends he has found it necessary somewhat to alter the usual language employed by optical writers;—thus, instead of speaking of the *focal length* of lenses, or the *radii of their surfaces*, he speaks of their *powers* and *curvatures*; designating, by the former expression, the quotient of unity by the number of parts of any scale which the focal length is equal to; and by the latter, the quotient similarly derived from the radius in question.

After adverting to some other parts of the subject of this paper, more especially to the problem of the destruction of the spherical aberration in a double or multiple lens, and to the difficulties which it involves, Mr. Herschel observes, that one condition, hitherto unaccountably overlooked, is forced upon our attention by the nature of the formulæ of aberration given in this paper; namely, its destruction not only from parallel rays, but also from rays diverging from a point at any finite distance, and which is required in a perfect telescope for land objects, and is of considerable advantage in those for astronomical use: 1st, The very moderate curvatures required for the surfaces; 2nd, That in this construction the curvatures of the two exterior surfaces of the compound lens of given focal length vary within very narrow limits, by any variation in either the refractive or dispersive powers at all likely to occur in practice; 3rd, That the two interior surfaces always approach so nearly to coincidence, that no considerable practical error can arise from neglecting their difference, and figuring them on tools of equal radii.

An Account of the Skeletons of the Dugong, Two-horned Rhinoceros, and Tapir of Sumatra, sent to England by Sir Thomas Stamford Raffles, *Governor of Bencoolen. By Sir* Everard Home, *Bart. V.P.R.S.* Read March 22, 1821. [*Phil. Trans.* 1821, *p.* 268.]

In this paper, Sir Everard first describes, by reference to an annexed drawing, the peculiar form of the skeleton of the dugong, which he compares to a boat without a keel, with the bottom uppermost; so that in the sea the middle of the back is the highest point in the water; and as the lungs are very extensive, they render the animal buoyant. As a compensation for legs, the dugong has a peculiar means of suspending itself in the sea, the centre of the back forming the point of suspension, similar to the fulcrum of a pair of scales; this peculiarity explains the form of the jaws, which are placed at an angle with the skull, unlike those of any other animal.

There is no remarkable difference between the bones of the two-horned rhinoceros, compared with those of the single-horned species, except that the projection in front of the skull, formed by the union of the nasal bones, is more nearly in a straight line and more ex-

tended; in the viscera of these animals there is, however, a more marked difference.

The bones of the Sumatran tapir closely resemble those of the American, but the skull of the former has a broader frontal bone, and no middle ridge; the nasal bones are larger, giving a proportionate increased dimension to the nostrils. The skeleton of the tapir differs from that of the rhinoceros in the smaller extent of the scapulæ and pelvis.

The stomach of the Sumatran tapir is shaped like that of the rhinoceros; the œsophagus is smooth and cuticular; the small intestines are 69 feet long; the length and greatest breadth of the cæcum is 1 foot; the length of the colon and rectum is 19 feet 6 inches; the spleen is long and narrow; the kidneys conglobate; and the lungs composed of one principal lobe on each side, of considerable length, and two smaller lobes.

On the Mean Density of the Earth. By Dr. Charles Hutton, *F.R.S.* Read April 5, 1821. [*Phil. Trans.* 1821, *p.* 276.]

Since the first notice of the determination of the mean density of the earth by Newton, two experimental inquiries only have been undertaken in relation to it; namely, in the case of the Schehallien experiment by the author and by Dr. Maskelyne; and by Mr. Cavendish, who used a method invented by Mr. Mitchell.

Dr. Hutton proposes in this paper to show by a statement of, and observations upon, the two methods, that the preference, in point of accuracy, belongs to the mountain experiment over that of the small balls employed by Mr. Cavendish; and the results of this experiment, duly corrected by that of Mr. Playfair's lithological survey of the mountain, give the mean density of the earth equal to 5 times the density of water, and not 4·5, a number unfairly assumed on some occasions, as the author's final determination.

In adverting to the advantage that might result from a repetition of the mountain experiment in some other favourable situation, and with improved means, Dr. Hutton suggests the employment of one of the large pyramids of Egypt for the purpose. The mass, he says, is sufficiently large, and the station for the plummet or zenith sector might be taken much nearer the centre of the mass than on a mountain, which would give a larger quantity of deviation of the plummet.

The regular figure and known composition of the mass would also yield facilities in calculating its attraction; and, moreover, the deviation of the plummet might be observed on all four sides.

On the Separation of Iron from other Metals. By J. F. W. Herschel, *Esq. F.R.S.* Read April 5, 1821. [*Phil. Trans.* 1821, *p.* 293.

After adverting to the importance of an easy means of effecting the above purpose in analytical inquiries, and to the insufficiency of

the usual methods hitherto described, the author proposes the following process. The solution containing the iron is to be peroxidized by nitric acid, then neutralized while boiling by carbonate of ammonium; the iron falls, while the other metals, which Mr. Herschel supposes to be manganese, cerium, nickel, and cobalt, remain in solution. A few precautions are necessary to insure success in this operation; such as, that the solution must contain no oxide of manganese or cerium, except in their states of protoxide; and that during the precipitation the solution should be duly diluted and agitated; and the latter portions of the alkaline solution, carefully added so as to avoid its excess, though slightly surpassing the point of saturation, give rise to no error or inconvenience.

Mr. Herschel concludes this paper with some observations respecting those peculiarities of the peroxide upon which its separation in the above cases depends, and gives some instances of its application to practical analysis.

On the Re-establishment of a Canal in the Place of a Portion of the Urethra which had been destroyed. By Henry Earle, *Esq. Surgeon to the Foundling, and Assistant Surgeon to St. Bartholomew's Hospital. Communicated by Sir* Humphry Davy, *Bart. P.R.S.* Read April 12, 1821. [*Phil. Trans.* 1821, *p.* 300.]

In this paper Mr. Earle details the case of a man whose urethra was much injured in the perineum by a fall in the year 1813, and who continued to suffer difficulty of making water till 1819, when he was attacked with retention of urine, followed by effusion and mortification; by which the integuments of the perineum, and more than an inch of the canal of the urethra, sloughed away; forming afterwards a large smooth cicatrix, above and below which the mucous membrane was still visible. After properly dilating the anterior part of the urethra, Mr. Earle performed the following operation:—A portion of integument was removed, about 1½ inch long and ½ inch wide, on the left side of the cicatrix; an incision was then made across the perineum, so as to pare away the callous edges of the urethra, and the cutis dissected from a portion of the integument on the right side of the perineum, leaving a smooth space between the cut surfaces to form the lining of the new canal. The integuments on the right side were then dissected up, turned over a catheter, and brought in contact with the opposite groove, being kept in their place by two ligatures, some straps of adhesive plaster, and a bandage. This first operation was attended with partial success; and the patient's general health being disordered, nothing further was done till the summer of 1820, when a second operation was performed as follows:—A deep groove was made on the right side of the surface denuded of its cutis; a portion of integument was then detached from the left side, and properly retained by the quill suture and adhesive plaster. About two thirds of the canal were thus completed; and by a third operation, upon a smaller scale, the cure was ultimately

effected; so that in March 1821 the parts were fit for the performance of their natural functions.

Calculations of some Observations of the Solar Eclipse on the 7th of September, 1820. *By Mr.* Charles Rumker. *Communicated by* Thomas Young, *M.D. For. Sec. R.S.* Read May 10, 1821. [*Phil. Trans.* 1821, *p.* 311.]

These calculations are founded upon Burckhardt's lunar, and Carlini's solar tables.

An Account of the Re-measurement of the Cube, Cylinder, and Sphere, used by the late Sir George Shuckburgh Evelyn, *in his Inquiries respecting a Standard of Weights and Measures. By Captain* Henry Kater, *F.R.S.* Read June 7, 1821. [*Phil. Trans.* 1821, *p.* 316.]

The experiments above adverted to are detailed in the Philosophical Transactions for 1798; and though the greatest attention was bestowed on those parts of the inquiry relating to the *weight* of the solids, the method of *measuring* them is not so fully detailed; Captain Kater, therefore, was desirous of re-investigating the latter subject before the Commissioners of Weights and Measures should make their final report.

The author then proceeds to describe the state of the apparatus; and the means which he adopted in effecting this measurement of three sides of the cube gives for its content 124·1969 inches.

The length of the cylinder deduced from these means is =5·9960 inches.

In measuring the sphere, a brass square was originally employed, the side of which was a little longer than the diameter. The sphere being properly placed and supported within the square, a micrometer screw which passed through one of the sides of the square was brought in contact with the diameter of the sphere, and the reading of the micrometer head noted; the sphere being then removed, a brass rule of known length was put into its place, and the micrometer screw being brought in contact with the end of the rule, the difference between its length and the diameter of the sphere was obtained, from which the latter was determined. Captain Kater details at length the repetition of Sir George's measurements, where it appears that the excess of the diameter of the sphere above the length of the rule gives 0·0012281 inch. The author then proceeded to measure the brass rule, the length of which was found equal to 6·0063609 inches; and the diameter of the sphere thence deduced gave 113·5264 inches for its solid content.

Captain Kater concludes this paper with a table, showing the data furnished by Sir George Shuckburgh Evelyn's experiments and his own measurements; from which it appears that the weight of a cubic inch of distilled water, in a vacuum of 62°, = 252·888 grains of Sir George's standard, or = 252·722 grains of the parliamentary standard.

An Account of Observations made with the Eight-Feet Astronomical Circle, at the Observatory of Trinity College, Dublin, since the Beginning of the Year 1818, *for Investigating the Effects of Parallax and Aberration on the Places of certain fixed Stars; also the Comparison of these with former Observations for determining the Effects of Lunar Nutation. By the Reverend* John Brinkley, *D.D. F.R.S. and M.R.I.A. Andrews Professor of Astronomy in the University of Dublin.* Read June 21, 1821. [*Phil. Trans.* 1821, *p.* 327.]

The observations, of which the details are given in this communication, were instituted with a view to discover the source of the differences that have existed between the observations made at Greenwich and those at Dublin. Dr. Brinkley's former observations of certain stars pointed out a deviation of about one second from the mean place, after having made all usual corrections. Mr. Pond's observations pointed out no such deviations. Dr. Brinkley referred the deviations which he had observed to parallax; and his principal object in the present paper is to ascertain how far that opinion is correct, and to show that no change of figure in the instrument, nor any uncertainties of the changes of refraction, can be admitted as the causes of the effects observed.

As, in deducing the quantity of parallax, the results must be affected by any uncertainty in the constant of aberration; and, in like manner, as in investigating the constant of aberration, from observations of a given star, the parallax, if any, will be involved,—the author adopted the following process in reducing the observations.

The observed zenith distances of a given star were reduced to January 1st, 1819, by the common equations, taking the constant of aberration $= 20''\cdot25$. The mean of these was taken. The correct mean zenith distance was supposed equal to this mean $- e$; the constant of aberration $= 20''\cdot25 + x$; and the semi-parallax $= p$. The equations of condition thus resulting from the respective observations, thus contained three unknown quantities. These equations were reduced to three by the method of making the sum of the squares of the errors a minimum. The solutions of these three equations give the values of e and p, and thence the values of the mean polar distance, constant of aberration, and semi-parallax.

After a very detailed account of the method of conducting his observations, instituted with a view of obtaining an explanation of the source of the difference of the results of his former observations, and of those of Mr. Pond relative to parallax, Dr. Brinkley states his inability to detect any such explanation, or to obtain any result opposed to his former conclusions: he remarks, however, that the discordances between his observations and those made at Greenwich, may by some be considered as showing the great precision of modern observations, since the whole extent of the absolute difference is only about one second. Independent, however, of the interest of the question of parallax, it is important, he observes, to ascertain the origin of this small difference.

On the Effects produced in the Rates of Chronometers by the Proximity of Masses of Iron. By Peter Barlow, *Esq. of the Royal Military Academy.* *Communicated by* John Barrow. *Esq. F.R.S.* Read June 28, 1821. [*Phil. Trans.* 1821, *p.* 361.]

It having been ascertained that during Captain Buchan's voyage to the arctic regions, in 1818, the rates of chronometers differed on board and on shore, and this change having been attributed by Mr. Fisher to the iron of the vessel, the author felt desirous of examining into the effects of the proximity of masses of iron upon chronometers' rates, and of determining their causes. In this communication he details the results of a variety of experiments and observations relative to the effects of iron on chronometers placed under different circumstances in its vicinity; whence he concludes, that it undoubtedly does alter their rates, but that it does not necessarily accelerate them, as suggested by Mr. Fisher: on the contrary, in Mr. Barlow's experiments, it was generally productive of retardation, much depending upon the direction of the balance in respect to the iron; and although the law of this influence has not been ascertained, it is suggested as a practical conclusion, that on ship-board care should be taken to keep chronometers out of the vicinity of any considerable mass or surface of iron; and as much of the iron of a ship is concealed, Mr. Barlow thinks the best way of ascertaining the best situation for a chronometer would be to set down a compass in any place designed for it, and to observe and compare the direction of a needle with that of the azimuthal compass on deck, while the vessel is on different tacks: and if the disagreement between the two be very considerable, another place should be chosen.

Lastly, as the power of the iron to disturb the chronometer resides on the surface (as in the instance of the compass), and as we know generally the distance and direction of a plate, such that its power may be equal to the mean action of the iron of the vessel, we are thence able to ascertain, before a chronometer is sent on board, whether the effect of the ship's iron will be to accelerate or retard its going.

On the Peculiarities that distinguish the Manatee of the West Indies from the Dugong of the East Indian Seas. *By Sir* Everard Home, *Bart. V.P.R.S.* Read July 12, 1821. [*Phil. Trans.* 1821, *p.* 390.]

The following are the principal differences pointed out by Sir Everard Home as characterizing the Manatee of the West Indies from that species of the Dugong, lately described to the Society, from Sumatra. It differs externally in the shape of the tail, in having neither tusks nor nails, and also in the form of its snout. The teeth differ in number; and though the general form of the skeleton is similar, there are fewer vertebræ. The stomach differs in the shape of the solid glandular part, and of the lateral pouches, but both animals feed upon fuci The forms of the teeth, however, in

these two species are totally different, which shows, says the author, how inefficient a mode of classing animals is furnished by the appearance of the teeth.

On a New Compound of Chlorine and Carbon. By Richard Phillips, *F.R.S.E. F.L.S. M.G.S. &c. and* Michael Faraday, *Chemical Assistant in the Royal Institution. Communicated by Sir* Humphry Davy, *Bart. P.R.S.* Read July 12, 1821. [*Phil. Trans.* 1821, *p.* 392.]

The above substance was discovered by M. Julien, of Abo, in Finland, amongst the products arising out of the distillation of calcined sulphate of iron, with crude nitre in iron retorts. It forms white acicular crystals by sublimation, and when passed through a green glass tube containing red-hot rock crystal, it is decomposed with the deposition of charcoal and evolution of chlorine. It is not altered by repeated sublimations in chlorine. It was analysed by passing its vapour over red-hot oxide of copper, by which chloride of copper and carbonic acid gas were produced: the former was decomposed by nitrate of silver, and the proportion of chlorine estimated by that of chloride of silver formed. From this and other experiments, the authors conclude that this substance consists of one portion of chlorine and two of carbon: they failed in their endeavours to convert it into either of the other chlorides of carbon, to which, in its physical and chemical properties, it bears however a considerable resemblance.

On the Nerves; giving an Account of some Experiments on their Structure and Functions, which lead to a new Arrangement of the System. By Charles Bell, *Esq. Communicated by Sir* Humphry Davy, *Bart. P.R.S.* Read July 12, 1821. [*Phil. Trans.* 1821, *p.* 398.]

In this paper the author proposes to limit his inquiry to the nerves of respiration, comprehending under that term all the nerves which serve to combine the muscles employed in the act of breathing and of speaking; and after showing that the simplicity or complexity of the nerves are as the functions or organizations of the parts which they supply, and that, however numerous and complex they appear in some parts of the body, they may nevertheless be divided into two distinct classes, by ascertaining what parts are necessary to life and motion, and what are superadded as requisite to higher and more complex enjoyments and actions; the former class comprehending the nerves of the spine, the suboccipital or tenth, and the trigeminus or fifth; and the latter the eighth pair, the portio dura of the seventh, the spinal accessory, the phrenic, the external respiratory, and the lingual; Mr. Bell proceeds to a detailed account of these nerves, showing, by an examination of the nerves of the face, that the two sets differ in structure and sensibility as

well as in function, and illustrating his positions by a variety of experiments, which, with their results, are given at length in this paper; and after showing the practical applications of which the investigations contained in it are susceptible, he concludes by observing, that a just estimate of their importance can scarcely be formed, till an analogous account of the nerves of the throat, neck, and chest shall be laid before the Society, which will show that in them also there are the same distinctions of structure and functions, and that the nerves of respiration may be distinguished and separated amidst the apparent intricacy of the general system, and that by dividing them, the motions of the several parts, which unite in the act of respiration, may be successively stopped; while their other functions, dependent upon their other nerves, are continued.

By pursuing this investigation, the remaining parts of the nervous system are also much simplified, and the apparent confusion arising out of the crossing and re-union of nerves is thus shown to be for the purpose of associating the muscles into different classes, for combining them in subserviency to different organs, and placing them under the guidance of a sensibility more certain in its operation than the will.

Further Researches on the Magnetic Phenomena produced by Electricity; with some New Experiments on the Properties of Electrified Bodies in their Relations to Conducting Powers and Temperature. By Sir Humphry Davy, *Bart. P.R.S.* Read July 5, 1821. [*Phil. Trans.* 1821, *p.* 425.]

In this paper Sir Humphry Davy adds to his former details upon the subject of electro-magnetism, by tracing the general effects of the action of electricity on conductors, in their relation to this new property and to heat.

The magnetic phenomena he found the same, whether the electricity was small in quantity, and passing through good conductors of great magnitude, or whether the conductors were so imperfect as only to convey a small quantity of electricity. That these magnetic powers are not affected by the mobility of the parts of fluids, Sir Humphry proved by the electrization of mercury and fusible metal, in glass tubes, which were thus made to attract iron filings and magnetic needles, while imperfectly conducting fluids did not, under similar circumstances, give any polarity to steel. Electricity passed through air, however, produces this effect; and Sir Humphry has succeeded in affecting the arc of fire by the approximation of a magnet.

In investigating the relative conducting powers of substances for electricity, Sir Humphry found that a wire kept cool is a better conductor than when heated; and the knowledge of this fact led to the explanation of a very singular result, namely, that by applying heat to one part of a wire in the circuit, its other parts become colder, and that by applying cold they become hotter; thus, when one part

of a wire, heated to dull redness in the circuit, is cooled by ice, the remaining portion becomes white hot, whilst the application of the flame of a spirit-lamp renders the other part colder.

In discussing the relations of heat, magnetism, and chemical action, to electricity, Sir Humphry particularly adverts to the relative elevations of temperature which the different metals undergo during the transmission of electricity: thus, when a chain, composed of alternate lengths of silver and platinum is made the connecting medium between the poles of a powerful battery, the silver wire being four or five times the diameter of the platinum, the former metal is not sensibly heated, whilst the latter becomes intensely ignited. Now if heat be regarded as *material*, we cannot suppose that it is expelled from the platinum because it may be thus generated indefinitely; again, if dependent upon, or identical with, electricity, its quantity should be similar throughout the metallic chain. In regard to the magnetism of this chain, the case is different, for every part of it exhibits equal magnetic powers; so that the power appears directly as the quantity of electricity.

Sir Humphry Davy concludes this communication with some general remarks respecting the different phenomena produced by the agency of electricity; whether they depend upon one or more species of ethereal matter, or whether they are merely exhibitions of the attractive powers and subtile motions of the corpuscles of common matter, are questions which remain for the determination of future researches and experiments.

The Bakerian Lecture. An Account of Experiments to determine the Amount of the Dip of the Magnetic Needle in London, in August 1821; *with Remarks on the Instruments which are usually employed in such Determinations. By Captain* Edward Sabine, *of the Royal Regiment of Artillery, F.R.S.* Read November 22, 1821. [*Phil. Trans.* 1822, *p.* 1.]

After describing the imperfections of the instruments in general use for ascertaining the dip of the magnetic needle, and adverting to the consequent inaccuracy and insufficiency of the observations made with them, Captain Sabine gives an account of the form of dipping-needle which he preferred for his experiments, and which was constructed for him by Mr. Dollond, upon principles laid down by Professor Meyer, of Gottingen. He then enters into minute details of the mode of pursuing and verifying his observations, the results of which, gained by three different methods, are as follows: viz. by 10 experiments with Meyer's needle, 70° 2′·9; by the times of oscillation in the magnetic meridian, and in the plane perpendicular to it (mean by three needles), 70° 04′; by the times of vertical and horizontal oscillation, 7° 02′·6. So that 70° 03′ may be considered as the mean dip of the needle towards the north, in August and September 1821, within four hours of noon, being the limit within which all the experiments were made.

Alluding to former observations for the purpose of determining the dip in London, the author observes that, independent of any imperfection in the instruments, they were made in houses in close built parts of the metropolis, and, therefore, all subject to the influence of local attraction; and, moreover, that the correction found by observing the difference of the dip on the outside of the house cannot be regarded as an effectual remedy, inasmuch as the needle may still have been attracted by iron in the adjoining houses, or in the neighbourhood. It is, indeed, only requisite to try needles in different situations in a city, to be convinced how little dependence should be placed in the accuracy of such results: the author thinks that it is rather owing to this cause than to instrumental error, that the dip at the Apartments of the Royal Society is stated in the Philosophical Transactions for the present year to be 71° 06′. To avoid this source of error, Captain Sabine conducted the observations which form the subject of this lecture in the nursery-ground in the Regent's Park, a situation which he regards in all respects eligible, and far removed from the influence of iron.

Some Positions respecting the Influence of the Voltaic Battery in obviating the Effects of the Division of the Eighth Pair of Nerves. Drawn up by A. P. Wilson Philip, *M.D. F.R.S. Edinb. Communicated by* B. C. Brodie, *Esq. F.R.S.* Read July 5, 1821. [*Phil. Trans.* 1822, *p.* 22.]

The positions established by Dr. Philip, to the satisfaction of Mr. Brodie, are detailed in this paper in the following order.

First. When the nerves are divided and the ends not displaced, if the animal live some hours, food swallowed *before* the operation is *much* digested; but if the ends of the nerves be turned from each other, no *perfectly digested* food is, under the same circumstances, found in the stomach, nor does digestion go on though the animal live; but galvanism applied to the nerves occasions a degree of digestion in the food contained in the stomach, and when galvanized the animal does not suffer from dyspnœa. When the nerves are simply divided, and the animal lives for six hours, the lungs become congested; but they appear healthy when galvanism has before been sent through the lower portion of the divided nerves.

On some Alvine Concretions found in the Colon of a young Man in Lancashire, after Death. By J. G. Children, *Esq. F.R.S. &c. &c. Communicated by the Society for Promoting Animal Chemistry.* Read December 13, 1821. [*Phil. Trans.* 1822, *p.* 24.]

After detailing the above, and adverting to two other cases of intestinal concretions, Mr. Children describes the appearance and composition of the calculi. The nucleus of each was a plumstone enveloped in a compact coating of phosphate of lime and ammoniaco-magnesian phosphate, and of a fibrous substance alternating in lay-

ers; the animal matter which they contain is chiefly gelatine and a little resin; and the fibrous vegetable matter appears to have been derived from the inner coat enveloping the farinaceous part of the oat. From other cases, which the author mentions at the conclusion of his paper, it appears that oatmeal has not unfrequently contributed to the deposition upon intestinal concretions; and from the analyses which he quotes, the same fibrous matter has been detected in them by other chemists.

On the Concentric Adjustment of a Triple Object-Glass. By William Hyde Wollaston, *M.D. V.P.R.S.* Read December 13, 1821. [*Phil. Trans.* 1822, *p.* 32.]

The centering of a triple achromatic object-glass has always presented considerable difficulties to practical opticians, which Dr. Wollaston has succeeded in removing, with regard to an excellent telescope in his own possession, by observing the relative position of the fifteen small images of a luminous object near the eye-glass, which are formed by the binary combinations of the reflexions of the six surfaces concerned, and which are seen by an eye situated beyond the object-glass, and assisted, if required, by a lens. When these images are all in the same right line, it is obvious that the glasses are not only well adjusted together, but that each is well centered; and by means of four screws acting on each glass, Dr. Wollaston was able to make the adjustment so complete, as considerably to improve the powers of the instrument.

On a New Species of Rhinoceros found in the interior of Africa, the Skull of which bears a close Resemblance to that found in a Fossil State in Siberia and other Countries. By Sir Everard Home, *Bart. V.P.R.S.* Read December 13, 1821. [*Phil. Trans.* 1822, *p.* 38.]

The animal described in this paper was shot about 300 miles west of La Goa Bay, 6 miles from the city of Mashow, and 1000 miles in a straight direction from the Cape of Good Hope. It is graminivorous, and not gregarious. The skull is 36 inches long; and the position of the horns, though differing in many respects from those of other existing species, bear so close a resemblance to those of the fossil skulls from Siberia, as to leave no prominent characteristic mark between them; hence the author doubts whether many races of animals supposed to be extinct are really so: he thinks it probable that they may have retired to uninhabited parts of the globe. The small capacity of the skull of the rhinoceros, as compared with that of the elephant, accounts for its inferior intellect, for the extreme difficulty of rendering the animal tractable, and for the failure of all attempts that have been made at taming him; and renders it not improbable that the unicorn, not to be tamed, mentioned by Job, is identical with the rhinoceros, since no other animal so devoid of intellect has ever been described. In that age, says the author, the short horn, which

cannot be regarded as a defensive weapon, might easily have been overlooked, and the smoothness of the skin would give it a greater resemblance to the horse than any other animal.

This paper is accompanied by two plates, exhibiting the skull of the African and of the fossil Siberian rhinoceros, and a fossil horn of the latter.

Extract of a Letter from Captain Basil Hall, *R.N. F.R.S. to* William Hyde Wollaston, *M.D. V.P.R.S. containing Observations of a Comet seen at Valparaiso.* Read January 10, 1822. [*Phil. Trans.* 1822, *p.* 46.]

The comet described in Captain Hall's letter was visible for 33 days in the months of April and May, 1821. During the first week its nucleus was bright and distinct; but being then in the interior of the country, he did not commence observing it till the 8th of April, when its nucleus had become so indistinct as to render its measurement by the micrometer uncertain. On its first appearance, the comet appeared of a dull white, and its tail presented a dark streak between its sides, giving it the appearance of being split. On the second evening the tail subtended an angle of 7°, reaching to ρ Ceti; on the seventh the nucleus was less bright, and the tail shorter, arising, probably, from the increased distance of the comet. The tail was at first nearly at right angles to the horizon, but each succeeding night it inclined more to the south. Tables of the observations and some sketches of the appearance of this comet accompany Captain Hall's communication.

Elements of Captain Hall's *Comet. By* J. Brinkley, *D.D. F.R.S. and M.R.I.A. and Andrews Professor of Astronomy in the University of Dublin. In a Letter addressed to* W. H. Wollaston, *M.D. V.P.R.S.* Read January 10, 1822. [*Phil. Trans.* 1822, *p.* 50.]

Dr. Brinkley remarks that the comet observed by Captain Hall is interesting to astronomers on account of its small perihelion distance, for there are only three, out of 116, in M. Delambre's catalogue, that pass nearer to the sun. On the 8th of April it was distant from the earth 1·41, and on the 3rd of May, 2·64, the sun's distance from the earth being unity.

Dr. Brinkley also remarks that it is probably the same comet that was observed in 1593; it agrees with that in its small perihelion distance, and great inclination. Of that comet, the inclination was 88°, and the perihelion distance ·089; of this, the inclination is 106° 44′, and its perihelion distance ·093.

To the proximity of this comet to the sun, when on the north side of the ecliptic, in February and March last, before it passed its perihelion, Dr. Brinkley attributes its having escaped European observers. It was never more than a few degrees from the sun, and therefore could not have been visible. The author then points out

the unusual circumstances relative to this comet, which have involved the computation of its elements in difficulties not often met with, and which induce him to request Dr. Wollaston to lay the method by which he proceeded before the Royal Society.

On the Electrical Phenomena exhibited in Vacuo. By Sir Humphry Davy, *Bart. P.R.S.* Read December 20, 1821. [*Phil. Trans.* 1822, *p.* 64.]

The relations of electricity to space, as nearly void of matter as it can be made on the earth's surface, are connected with many important queries bearing upon the nature of heat, light, electricity, and magnetism.

The vacuum used by Sir Humphry Davy was that above the mercury in the barometer tube, and a more perfect one produced in the same way by fused tin; the former he found always permeable to electricity, but the colour and intensity of the light in traversing the mercurial atmosphere was remarkably affected by its temperature; it became green and vivid when the tube was heated, and was scarcely perceptible in a very dark room, when it was cooled to 0°; which phenomena, as well as some others described by the author, are referable to the varying density of the mercurial vapour. The admission of a little air rendered the light blue, and improved the conducting power of the medium. The most perfect vacuum that could be obtained above fused tin, was also permeable to electricity; but the light was yellow and exceedingly pale, and only slightly increased by heat. Electric and magnetic repulsions and attractions took place in the mercurial vacuum, as in air;—a circumstance which shows, says Sir Humphry, that they are not dependent upon elastic ponderable matter, and point them out as primary causes of other electrical phenomena.

From the aggregate results of his researches, the author thinks it evident that the light, and probably the heat, generated in electrical experiments, depend principally upon some properties or substances belonging to the ponderable matter through which it passes, and they render it probable that it is entirely owing to this source.

Croonian Lecture. On the Anatomical Structure of the Eye; illustrated by Microscopical Drawings, executed by F. Bauer, *Esq. By Sir* Everard Home, *Bart. V.P.R.S.* Read November 15, 1821. [*Phil. Trans.* 1822, *p.* 76.]

Having ascertained, by the aid of Mr. Bauer's microscopical observations, that neither the marsupium nor the ciliary processes are muscular, and therefore inadequate to those adjustments of the crystalline lens requisite for distinct vision; and that the structure of the choroid coat is also membranous,—the author turned his attention to the structure of the iris, which in the human eye resembles that of the quadruped developed by Mr. Maunoir in his Treatise on the

Artificial Pupil, consisting of an anterior membranous and a posterior muscular coat; the latter, in the act of contraction, presses back the lens upon the vitreous humour, the elasticity of which causes it again to advance when the sphincter muscle of the iris relaxes. In proof of the correctness of this opinion, Sir Everard adduces some experiments illustrative of the influence of belladonna upon myopic eyes.

Having ascertained that the marsupium is not muscular, I could only consider it, says the author, as a screen preventing the pencils of rays that fall upon the portion of the retina within the axis of the optic nerve, from extending to the outer portion. And after explaining to Mr. Dollond, that the situation of the bird's eye in the head makes the image of a distant object fall upon the retina within the axis of the optic nerve, and of a near one without that axis, as the bird only sees the object with one eye at a time; but the human eye, and that of quadrupeds, will have the image of a distant object fall on the retina, without that axis, both eyes being turned to the object;—he said that the inner portion of the bottom of the bird's eye was more extensive than the outer, and made a portion of a larger curve; consequently, was at a greater distance from the lens, and therefore adapted to longer pencils of rays fitting it for distant vision; while the outer portion being nearer, the lens was fitted for seeing near objects, the marsupium confining the rays, and preventing the vision from being confused.

In the human species and quadrupeds, the bottom of the eye has one uniform curve, the portion within the axis of the nerve being smaller than the outer, which is the very reverse of what it is found to be in birds, adapting both eyes to see the same distant object at one time.

A Letter from John Pond, *Esq. Astronomer Royal, to Sir* Humphry Davy, *Bart. President of the Royal Society, relative to a Derangement in the Mural Circle at the Royal Observatory.* Read November 22, 1821. [*Phil. Trans.* 1822, *p.* 86.]

On the Finite Extent of the Atmosphere. By William Hyde Wollaston, *M.D. V.P.R.S.* Read January 17, 1822. [*Phil. Trans.* 1822, *p.* 89.]

It has been inferred from measuring the barometrical pressure at different elevations, that the earth's atmosphere extends to the height of 40 miles, beyond which limit we are left to conjectures, founded on the supposed divisibility of matter; which, if infinite, indicates an infinity of atmosphere. If, however, it consist of ultimate indivisible particles, then expansion of the medium composed of them must cease at that point where the force of gravity downwards, upon a single particle, is equal to the resistance arising from the repulsive force of the medium. If we adopt the latter hypothesis, no part of our atmosphere could ever leave the earth; if the former, every pla-

netary body must possess its respective share, provided we rest our reasoning upon the known properties of matter. The author, therefore, thought it deserving of consideration, whether, in any instance, any deficiency of atmosphere could be proved, and whether, from such source, any conclusive argument could be drawn in favour of ultimate atoms of matter in general; for since the chemical law of definite proportions applies to all forms of matter, if it can be proved that any one body consists of particles no longer divisible, we then can scarcely doubt that all others are similarly constituted. In respect to the non-existence of an atmosphere round the moon, Dr. Wollaston observes, that the quantity of such matter retainable by a body of the moon's magnitude, could not give rise to any phenomena observable by our instruments; that we should therefore look for information in an opposite direction, and examine that body which has the greatest power; if we there find no appearance of an atmosphere, we may infer that our own is peculiar to the earth. In respect to the sun, for instance, if we calculate at what apparent distance from his body his force is equal to that of gravity at the surface of the earth, it is there that his power would be sufficient to accumulate, from an infinitely divisible medium filling all space, an atmosphere fully equal in density to our own, and therefore producing a refraction of more than 1° in the passage of rays obliquely through it.

Dr. Wollaston then proceeds to show, from a detail of observations of the passage of Venus near the sun in superior conjunction, which took place in May last, that no such retardation in the motion of that planet could be perceived in her progress toward the sun, as would occur from increasing refraction, and that the phenomenon does not offer the least evidence of the existence of a solar atmosphere.

After some suggestions respecting the best means of pursuing this investigation, Dr. Wollaston observes, that he has dwelt perhaps more upon the consideration of a solar atmosphere than may seem necessary to those who will consider the phenomena of the occultations of Jupiter's satellites by the body of the planet, the approach of which is regular, till they appear in contact, instead of being retarded by the refraction arising from an atmosphere so extensive as Jupiter should attract to himself, from an infinitely divisible medium filling space.

Since the mass of Jupiter is 309 times that of the earth, the distance at which his attraction is equal to gravity must be about 17·6 times the earth's radius; and since his diameter is nearly 11 times greater than that of the earth, 1·6 time his own radius will be the distance from his centre at which an atmosphere equal to our own should occasion a refraction exceeding 1°. To the fourth satellite this distance would subtend an angle of about 3° 37′; so that an increase of density to 3·5 times that of our common atmosphere would be more than sufficient to render the fourth satellite visible when behind the centre of the planet, and consequently to make it appear on all sides at

the same time. The space of about six miles in depth, within which this increase of density could take place according to known laws of barometric pressure, would not subtend to our eye so much as $\frac{1}{100}$th of a second, a quantity not to be regarded in an estimate where so much latitude has been allowed for errors.

In concluding this paper, Dr. Wollaston remarks, that although in reference to a solar atmosphere some doubt may be entertained in consequence of peculiar effects of heat, no such error can be suspected in regard to Jupiter; and as that planet has not its due share of an infinitely divisible atmosphere, there seems no ground upon which the phenomenon of the earth's atmosphere can be maintained, but on the supposition of ultimate atoms of definite magnitude, no longer divisible by repulsion of their parts.

On the Expansion in a Series of the Attraction of a Spheroid. By James Ivory, *M.A. F.R.S.* Read January 17, 1822. [*Phil. Trans.* 1822, *p.* 99.]

Mr. Ivory's principal object in this paper appears to be the removal of some difficulties in the demonstration of the method of developing the attractions of spheroids in an infinite series, as employed by Laplace in the *Mécanique Céleste*. It is natural to think, he observes, that the theory of the figure of the planets would be placed on a firmer basis if it were deduced directly from the general principles of the case, than when it is made to depend on a nice and somewhat uncertain point of analysis; and he conjectures that the theory will probably be found to hinge on this proposition,—that a spheroid, whether homogeneous or heterogeneous, cannot be in equilibrium by means of a rotatory motion about an axis, and the joint effect of the attraction of its own particles and of the other bodies of the system, unless its radius be a function of three rectangular coordinates; for if this proposition were clearly and rigorously demonstrated, the analysis of Laplace, on changing the ground on which it is built, would require little or no alteration in other respects.

Without, however, attempting to demonstrate this proposition in all its extent, the author has substituted a more direct and simple mode of argument than that of Laplace, which is perfectly conclusive with respect to all the cases to which the theorem in question can possibly require to be applied. He has shown that by immediately transforming a given expression into a function of three rectangular coordinates, we obtain the same development as is deduced in the *Mécanique Céleste*, by a more general and complicated mode of reasoning, which seems to be so far objectionable, as it tends to introduce a variety of quantities into the series which do not alter its total value, since they destroy each other, but which may possibly interfere with the accuracy of its application to particular cases, in which it may be employed as a symbolical representation: for example, when any finite number of terms is assumed as affording an approximate value:

since, if the expression developed has not been reduced to the form of a function of three rectangular coordinates, the development may contain an infinite number of terms, which are introduced by the operation without being essential to its final result. He takes for the example of such a case the equation of a spheroid, prominent between the equator and the poles, somewhat resembling the figure which was once attributed to Saturn; and he shows that its development in the form required will contain an infinite number of quantities arising from the expansion of a radical, which are not to be found in the original function.

Mr. Ivory considers, in the second place, the differential equation that takes place at the surface of a spheroid, and the demonstrations which have been published by Laplace and by Poisson; and he concludes that this equation is wanted neither for proving the possibility of the development, nor for calculating its terms; but in this plainer way of considering the matter, it appears that the development does not represent the given expression, when that expression is not an explicit function of three rectangular coordinates, in the same sense that it does when it is such a function. There is, therefore, a difficulty left unexplained; and we may be permitted to doubt whether so important a part of the celestial mechanics as that regarding the figure of the planets, rests with sufficient evidence on the doctrine laid down concerning the generality of the development.

On the late Extraordinary Depression of the Barometer. By Luke Howard, *Esq. F.R.S.* Read January 24, 1822. [*Phil. Trans.* 1822, *p.* 113.]

On the evening of the 24th of December last, Mr. Howard found the barometer at his house at Tottenham Green at 28·20 inches. The wind was moderate at S.E., the temperature 45°, and water boiled freely at 210°. At 11 P.M. the barometer fell to 27·96 inches, and at 5 A.M. on the 25th to 27·82, below which the author thinks it did not descend. By 8 A.M. it again reached 28. In the twenty-four hours preceding, 0·8 inch rain had fallen, but in the twenty-four hours following there fell none, nor was the wind strong. By midnight on the 25th the quicksilver reached 28·07 inches, and remained there during the twelve hours following,—a thing very rare in our climate. The quicksilver then rose in an uninterrupted curve, and on the 31st touched upon 30 inches, with fine weather. A diagram showing the state of the barometer during the last two months of 1821, as well as the quarter of the wind and quantity of rain fallen, accompanies this paper. During these two months the rain amounted to 10·10 inches, a quantity without precedent in the same space of time at London.

On the anomalous Magnetic Action of Hot Iron between the White and Blood-red Heat. By Peter Barlow, *Esq. of the Royal Military Academy. Communicated by Major* Thomas Colby, *of the Royal Engineers, F.R.S.* Read January 24, 1822. [*Phil. Trans.* 1822, *p.* 117.]

Finding the attractive power of soft malleable iron and steel for a magnet greater than that of cast-iron and hard steel, the author was desirous of ascertaining the effect of heating these bodies in a furnace, so as to render them perfectly soft, upon their magnetic power. With this view the bars were rendered white-hot, and being placed in the direction of the dip, their powers were found nearly equal. It was however found that there was a point between the white heat, at which all magnetic action was lost, and the blood-red heat, at which it was strongest, at which the iron attracted the needle the contrary way to which it did when cold; viz. if the bar and compass were so placed that the north end of the needle was drawn to it when cold, the south end was attracted during the interval abovementioned.

The author then proceeds to detail some further experiments in illustration of this anomalous magnetic action, from which it appears that the quantity of magnetic attraction at a red heat is influenced by the height or depth of the centre of the bar from the compass; and as the natural effect of the cold iron was changed by placing the compass below the centre of the bar, it became a question how far the negative attraction was also changed. To decide, the compass was lowered to within six inches of the bottom of the bar, when the cold iron produced a deviation of 21°, by attracting the south end of the needle. At a white heat its power ceased; but as this subsided to bright red the negative attraction amounted to 10½°, the north end of the needle being attracted to the iron; it then gradually returned to due north, and ultimately to 70° 30′ on the opposite side.

Mr. Barlow then gives the results of another series of experiments made with the bars inclined in the direction of the dipping-needle, showing that where the negative attraction was greatest the natural attraction was least, that is, opposite the middle of the bar, or in the place of no attraction.

Being doubtful how far the heat itself, independent of the iron, might be the cause of the anomalous action above described, the author substituted a heated copper bar for that of iron, but it produced no motion in the needle. He thinks it probable that the anomalies may depend upon the iron cooling faster towards its extremities than towards its centre, one part of the bar thus becoming magnetic before the other, and choosing a different species of attraction.

Observations for ascertaining the Length of the Pendulum at Madras in the East Indies, Latitude 13° 4′ 9″·1 *N.; with the Conclusions drawn from the same. By* John Goldingham, *Esq. F.R.S.* Read January 31, 1822. [*Phil. Trans.* 1822, *p.* 127.]

The observations detailed in this paper are comprised in two series. By the result of the first, the pendulum of experiment, which was constructed upon the same principles as that used by Captain Kater, and described in the Philosophical Transactions for 1819, was found to make 86166,108 vibrations in twenty-four hours, and by the result of the second series, 86166,048, the mean being 86166,078; so that the result of each series differs from the mean only $\frac{30}{1000}$th of a beat in twenty-four hours.

The length of the seconds pendulum at Madras, deduced as the mean of these two series of observations, is 39·026302 inches of Sir George Shuckburgh's scale, at the temperature of 70° in vacuo, and at the level of the sea.

By comparing this with the length of the pendulum vibrating seconds in London, we obtain $\frac{1}{304.56}$ as the ellipticity of the earth, which is very nearly the mean deduced from the observations of Captain Kater in England, and those of the French mathematicians.

Account of an Assemblage of Fossil Teeth and Bones of Elephant, Rhinoceros, Hippopotamus, Bear, Tiger, and Hyæna, and sixteen other Animals; discovered in a Cave at Kirkdale, Yorkshire, in the year 1821: *with a comparative View of five similar Caverns in various Parts of England, and others on the Continent. By the Rev.* William Buckland, *F.R.S. F.L.S. Vice President of the Geological Society of London, and Professor of Mineralogy and Geology in the University of Oxford, &c. &c. &c.* Read February 21, 1822. [*Phil. Trans.* 1822, *p.* 171.]

The rock in which the cavern, mentioned in the title of this paper, is formed, is that species of limestone called Oolite. Its greatest length is from 250 to 300 feet, and its breadth and height vary from two to seven feet, there being few places in which it is possible to stand upright. Its bottom was covered by a sediment of mud, and the roof and sides, as well as the surface of the mud, were incrusted by stalactitic matter. The animal remains were found, not upon the surface, but in the lower part only of this muddy deposit, and in the stalagmitic accumulations beneath it, and were thus remarkably preserved from decay. The teeth and bones hitherto discovered are those of the hyæna, fox, bear, of an animal of the tiger kind, of the elephant, rhinoceros, hippopotamus, and horse, of the ox and some species of deer, of the water rat and the rabbit. They were strewed promiscuously over the bottom of the cave; the bones, with very few exceptions, being broken and apparently gnawed: for upon many of them marks were detected fitting the form of the canine teeth of the hyænas that were found there: whence it appears probable that this

was once a den of hyænas, who dragged into its recesses the other animal bodies, whose remains are mixed indiscriminately with their own; a conjecture, says the author, rendered almost certain, by the discovery of a portion of solid calcareous excrement, recognized by the keeper of the Menagerie at Exeter 'Change, from its resemblance to that of the Cape hyæna; the analysis, too, of this excrement shows its derivation from bones, as it consists chiefly of phosphate and carbonate of lime.

It appears from the researches of M. Cuvier, that the fossil hyæna was nearly one third larger than the largest of the modern species, of the habits of which the author gives an account, with a view of verifying and illustrating his opinion concerning the state and origin of the contents of the Yorkshire cave. Even the abundance of the remains of water rats, he says, is consistent with the omnivorous appetite of modern hyænas. In respect to ruminating animals, as they form the ordinary food of beasts of prey, the quantity of their bones is not surprising; but the abundant occurrence of some of the other remains, in a cave of the dimensions of that described, is not so obvious; since such animals as the elephant, rhinoceros, and hippopotamus, could not possibly have found an entrance, and since it is foreign to the habits of the hyæna to prey on the larger pachydermata. As a solution of this difficulty, the author supposes that the remains in question are those of individuals who died a natural death; and though the hyæna would neither have had strength to kill an elephant or rhinoceros, nor to drag home the entire carcase of a dead one, yet he might convey the most bulky animals piecemeal into his den, supposing them to have died in the neighbourhood. From this view of the subject it appears probable that the accumulation of these bones went on during a succession of years, while the animals in question were natives of this country; and the general dispersion of similar bones through the diluvian gravel of high latitudes, over a great part of the northern hemisphere, shows that the period at which they inhabited these regions was that immediately preceding the formation of this gravel, and that they perished by the waters that produced it. Moreover, as all these animals belong to species now unknown, and as there is no evidence of their ever having existed subsequent to the formation of the diluvium, we may conclude that the period at which the bones were introduced into the Kirkdale cave was antediluvian. That these extinct species never re-established themselves after the deluge, seems proved by the total absence of their remains in the varieties of postdiluvian accumulations of sand, mud, and peat, in which, however, we find the remains of horses, deer, and some other animals.

The phenomena, then, of this cave seem referable to a period at which the world was inhabited by land animals bearing only a general resemblance to those now existing, before the last inundation of the earth. So completely, however, had the violence of that tremendous convulsion destroyed and remodelled the form of its antediluvian surface, that it is only in caverns protected from its ravages that we may

hope to find undisturbed evidence of the events occurring in the immediately antecedent period; and such seem to be the bones and stalagmite formed before the introduction of the diluvian mud in the Kirkdale cave.

The author then proceeds to take a general view of the operations successively going on in this cave, founded upon his previous minute descriptions of its contents; of its earliest period when it was an unoccupied aperture; of its second period, when tenanted by hyænas, during which, as well as the former, stalagmite and stalactite were forming, though necessarily interrupted by the ingress and egress of its lodgers; of the third period, when the animals were extirpated and the mud introduced, and which appears therefore to have been that of the deluge; and of the fourth and last period, during which the superficial stalagmite incrusting the mud was formed, and in which no creature appears to have entered the cave till it was opened last summer, and no other process to have been going on than the uninterrupted formation of stalactite.

The strata of diluvial sediment seem to mark the point of time at which the latter state of things began, and the former ceased; and the limited quantity of postdiluvian stalactite, no less than the undecayed condition of the bones, tends to show that the time elapsed since the introduction of the mud has not been of excessive length.

Professor Buckland concludes this paper with an application of the arguments arising out of the detail of facts which it contains, to the illustration of other similar phenomena, where the evidence of their origin is less complete; of these, five are in our own country, and the author avails himself of their history, together with that of the Kirkdale cave, to elucidate the account of analogous caverns, which have been more or less perfectly examined in various parts of the Continent of Europe.

Communication of a curious Appearance lately observed upon the Moon. By the Rev. Fearon Fallows. *In a Letter addressed to* John Barrow, *Esq. F.R.S.* Read February 28, 1822. [*Phil. Trans.* 1822, *p.* 237.]

On the evening of the preceding 28th November, Mr. Fallows observed a luminous spot on the dark part of the moon's limb, which, by aid of a four-feet achromatic telescope, of a power of 100, seemed like a star of the sixth magnitude; three others much smaller were also remarked, but want of proper instruments prevented Mr. Fallows from ascertaining their situation. On the 29th, the large spot was as brilliant as on the preceding evening, the other two nearly invisible, and the third and most brilliant of the small spots had disappeared. On the 30th, the weather prevented further observations.

On the Difference in the Appearance of the Teeth and the Shape of the Skull in different Species of Seals. By Sir Everard Home, *Bart. V.P.R.S.* Read February 28, 1822. [*Phil. Trans.* 1822, *p.* 239.]

In this paper, which is illustrated by three drawings, Sir Everard brings before the Society an account of some peculiarities in the skull and teeth of different species of Seals, in order to prevent mistakes being made when fossil remains of that animal are met with. The first drawing is of the skull of the large seal, from the South Seas; the second, from a seal shot near the Orkneys; and the third, from a seal's skull in the Museum of the College of Surgeons, from New Georgia, near the ice towards the South Pole. In all these the teeth differ in form, which the author suggests may arise from the shell fish on which they live being of different kinds.

Experiments and Observations on the Development of Magnetical Properties in Steel and Iron by Percussion. By William Scoresby, *Jun. Esq. Communicated by Sir* Humphry Davy, *Bart. P.R.S.* Read March 7, 1822. [*Phil. Trans.* 1822, *p.* 241.]

The result of the experiments detailed in this paper are, that merely hammering a bar of soft steel upon pewter and upon stone, gives it a feeble magnetic power; but that the same bar hammered vertically upon a poker, became much more powerfully magnetic. On inverting the bar a single blow nearly destroyed its magnetism, while the effect of two blows was to change the poles. Several blows upon the end of the bar when in the plane of the magnetic equator, also destroyed polarity. Increase in the length of the bars augmented the magnetism thus given by percussion. A strong magnet properly tempered was uniformly injured by hammering, but most rapidly when the north pole was upwards; after the magnetism, however, had been thus reduced to a certain extent, the power became nearly stationary, so that striking it in any position with the same hammer, occasioned scarcely any change of intensity.

The strong magnetizing effect of percussion on soft steel, induced Mr. Scoresby to apply the property to the formation of magnets, and he succeeded in giving to them considerable lifting powers, every care being taken to exclude all magnetic substances, and especially to free the bars of magnetism before the experiment.

The author also examined the effects of percussion upon soft steel magnets, and upon cast iron.

A soft steel magnet, lifting 1000 grains, when placed vertically upon a poker, with its north pole upwards, had its magnetism destroyed by five blows.

A bar of soft iron hammered vertically upon the poker could not be made to lift more than between 6 and 11 grains. A cast iron bar of the same size, and similarly treated, lifted 37 grains, and, having acquired this power, its magnetism was nearly destroyed by five blows with the north pole upwards.

On the Alloys of Steel. By J. Stodart, *Esq. F.R.S. and Mr.* M. Faraday, *Chemical Assistant in the Royal Institution. Communicated by* J. Stodart, *Esq. F.R.S.* Read March 21, 1822. [*Phil. Trans.* 1822, *p.* 253.]

The metals which form the most valuable alloys with steel, for the purpose of manufacture into cutting instruments, are silver, platinum, rhodium, iridium, osmium, and palladium. Eight pounds of Indian steel, alloyed with $\frac{1}{500}$th of pure silver, formed a very hard and tenacious compound, well adapted to the manufacture of cutlery and several edge tools. Ten pounds of the same steel with $\frac{1}{100}$th of pure platinum, produced an alloy less hard but more tough than the former.

With rhodium, iridium, and osmium the alloys were also excellent; but the scarcity of those metals prevents their general introduction into the manufactory.

The authors then state the processes of analysis which they adopted to assure themselves of the composition and perfection of the respective alloys.

The alloy with platinum is rapidly acted upon by sulphuric acid; and to ascertain the presence of the platinum, the residuary black powder of this solution, after having been heated red hot, is to be treated with nitro-muriatic acid, and the platinum reduced by heat.

The residuary matter of the alloy of silver, when similarly acted on by dilute sulphuric acid, was dissolved in nitric acid, and tested by muriatic. The residue of the alloy of palladium having been heated, was dissolved in nitro-muriatic acid, and precipitated by prussiate of mercury. From the rhodium residuum the iron was removed by muriatic acid, and long digestion in nitro-muriatic acid gave an easily distinguishable muriate of rhodium. The residue of iridium and osmium was decomposed in the usual way by the action of caustic soda.

The rapid action of dilute sulphuric acid upon these alloys, as compared with pure steel, is next adverted to, and is referred to electrical action, a voltaic combination being produced by the liberation of the difficultly oxidable metal.

The black insoluble residue of these alloys, obtained by the action of dilute sulphuric, appear to be peculiar compounds. When boiled in dilute muriatic acid, protoxide of iron is dissolved, and a black insoluble compound remains, which, when dried and heated to 400°, burns like pyrophorus, leaving protoxide of iron and the alloying metal: hence, say the authors, during the action of the acids on the steel, a portion of hydrogen enters into combination with part of the metal and charcoal, and forms an inflammable compound not acted on by the acids. The action of nitric acid on some of these residua produces a deflagrating compound, especially that from the alloy of platinum.

This paper concludes with some observations on the probable im-

portance of some triple alloys, and on the substitution of pure iron for steel in the formation of alloys.

Some Observations on the Buffy Coat of the Blood, &c. By John Davy, *M.D. F.R.S.* Read April 18, 1822. [*Phil. Trans.* 1822, *p.* 271.]

The peculiar appearance which the blood sometimes assumes after its coagulation, and which has been termed "buff," has been referred by Mr. Hewson to its increased tenuity and slow coagulation. Dr. Davy is inclined to ascribe the separation of the colouring matter from a part of the coagulum, or diminished viscidity to the former rather than the latter cause; for he has remarked that blood drawn in several inflammatory diseases coagulates with its usual rapidity, and yet forms the buffy coat: its specific gravity also rather exceeds than falls short of that of healthy blood.

Dr. Davy next adverts to the opinion that the age of the morbid adhesions, which on dissection are often found to connect together serous membranes, is proportionate to their strength: but as he has found strong adhesions formed in twenty-four hours between surfaces of the pleura, in consequence of inflammation artificially excited; and as he has remarked a peculiar tenacity in the coagulable lymph of the blood, enabling it to be drawn out into bands and fibres, which become solid and opake, and thus represent ordinary adhesions, he is inclined to doubt the correctness of the above opinion.

Lastly, the author mentions the supposed pouring out of serous fluids into certain cavities, after death; and cites experiments to show that, under ordinary circumstances, no such effusion or exudation of serum takes place.

On the Mechanism of the Spine. By Henry Earle, *Esq. F.R.S. Surgeon to the Foundling, and Assistant Surgeon to St. Bartholomew's Hospital.* Read April 25, 1822. [*Phil. Trans.* 1822, *p.* 276.]

In examining the structure of the vertebræ in different animals, the author was particularly struck with the mechanism of the spine and spinal canal in birds, by which great motion is gained in the neck without injury to the spinal marrow. The cervical vertebræ in birds vary from 9 to 24 in number; they differ considerably from each other, and are articulated by complex joints, in some respects resembling the articulation of the olecranon with the humerus in the human subject, but admitting also of lateral motion. The varying positions of the articulating surfaces are favoured by the interposition of a cartilage, adapted to the surface of each bone, and inclosed between reduplications of sinovial membrane; each joint is thus double, and resembles the articulation of the human lower jaw. The canal of each vertebra is contracted in the centre, and enlarges above and

below. In front it is closed by the bodies of the vertebræ; but in the skeleton its posterior part is imperfect, being filled up in the recent state by membrane, and protected by the ligamentum nuchæ.

The author then describes the appearance, on dissection, of the membranes of the spinal marrow, and of the mechanism by which its compression in the various and extensive motions of the neck is prevented; he also adverts to the exact correspondence between the extent of motion permitted, and the size and form of the canal in the human spine, and to some pathological consequences connected with such structure.

Drawings of some of the cervical vertebræ in birds, with a description of their different parts, are annexed to this paper.

Of the Nerves which associate the Muscles of the Chest, in the actions of Breathing, Speaking, and Expression. Being a Continuation of the Paper on the Structure and Functions of the Nerves. By Charles Bell, *Esq. Communicated by Sir* Humphry Davy, *Bart. LL.D. P.R.S.* Read May 2, 1822. [*Phil. Trans.* 1822, *p.* 284.]

This paper forms the continuation of that printed in the last volume of the Society's Transactions, by the same author. In the present communication the author proceeds to show that the office of the respiratory apparatus is not confined to the changes produced upon the blood, but that the same actions are employed in subservience to other organs, and that they perform a variety of functions, as in the natural voice in articulate language, and in the expression of passion, as well as in the more familiar acts of smelling, coughing, sneezing, &c.

Having established the proofs of the necessity of a number of remote parts being joined in the performance of these functions, he proceeds to show that there is a distinct class of nerves for this purpose. That these nerves depart from the same column of the spinal marrow, and diverge to all the parts of the frame, which are drawn into consent in the action of respiration.

Proceeding to show the difference betwixt the calm and uniform breathing for the purposes of circulation, and the excited and more irregular actions, as in speaking, singing, coughing, and sneezing, he proves the necessity of certain powerful muscles being brought in as accessories and aids to the common muscles of respiration. He nexts shows that the respiratory nerves are entirely distributed to these accessory muscles.

After tracing these nerves, and disengaging them from their intricate relation with the other nerves, he proceeds, by comparative anatomy, and by experiments, to show that they are respiratory nerves, and that their division cuts off the parts to which they are respectively distributed from participating in the act of respiration.

He takes occasion to show that authors have attended too exclusively to the par vagum, or eighth pair of nerves, which is only the principal or central nerve of an extensive class of nerves, which

have the same root, and receive their power from the same source; and that when injured in their common origin, there is a simultaneous cessation of motion in all the apparatus of respiration, that breathing instantly ceases, and with it life.

Touching on Pathology, he assigns reasons for believing that sudden death, when there is no apparent injury of vital organs, is by disorder of this division of the nervous system.

Having distinguished these nerves from the common voluntary and sensible nerves on the one hand, and from the sympathetic system on the other, he proceeds to show that expression is seated in these nerves. That they are not merely the nerves which order the motions of breathing, the nerves of natural and articulate language, but that through them the breast, chest, and face become the organs of expression, whenever the heart is agitated by sentiment or passion; and that without their instrumentality, the utmost agitation of the spirits in passion would be attended with no outward sign.

Experiments and Observations on the Newry Pitch-stone, and its Products, and on the Formation of Pumice. By the Right Hon. George Knox, *F.R.S.* Read May 9, 1822. [*Phil. Trans.* 1822, *p.* 313.]

After describing the geological locality and the external character of the above mineral, and adverting particularly to its oily smell, Mr. Knox proceeds to show, by its chemical analysis, that, exclusive of the constituents of this substance ascertained by Klaproth, it contains a considerable but variable proportion of a peculiar bitumen, separable from it by distillation at high temperatures. The author also succeeded in detecting some volatile principle in the pitch-stone of Meisser, analysed by Klaproth, as also in that of Arran; but it exists in them in smaller quantities than in the pitch-stone of Newry.

After having separated the water and bitumen from the mineral by heat, Mr. Knox found that by subjecting the residue to a bright red heat, it assumed not merely the appearance, but the properties of pumice; and he attributes this appearance to the slow escape of the bituminous matter, producing a vesicular structure.

The author details in this paper the process of analysis which he employed for the separation of the constituent parts of this pitch-stone, and adverts to those circumstances in which it appears to differ from the varieties of the mineral previously examined.

Observations on the Changes the Egg undergoes during Incubation in the common Fowl, illustrated by Microscopical Drawings. By Sir Everard Home, *Bart. V.P.R.S.* Read May 16, 1822. [*Phil. Trans.* 1822, *p.* 339.]

The molecule from which the future embryo is to be formed, is observed upon the surface of the yolk before it leaves the ovarium. It consists of globules $\frac{1}{2,800}$th of an inch in diameter, surrounded by a mixture of these and larger oval globules, similar to those of the

bird's blood, excepting their red colour; some oil is also discoverable. In the passage of the yolk along the oviduct, it acquires the albumen and its membrane; in this passage also the thread-like substances, which Mr. Hunter called the poles, were formed. Sir Everard next describes the changes which the egg undergoes during incubation. In four hours the rudiments of the embryo are perceptible; and in eight hours the brain and spinal marrow are surrounded by an amnion, all of which increase in distinctness for the first twenty-four hours. In thirty-six hours the intervertebral nerves and the lobular structure of the brain, and in forty-four hours the eye and heart are seen, and in two days and twelve hours it contained red blood, and arterial ramifications began to be formed. In three days the rudiments of the wings and legs were formed. These parts progressively increase until the sixth day, when the amnion is filled with water, and shortly afterwards the parietes of the thorax begin to form, and muscular action becomes evident. In seven days and twelve hours arterial pulsation was first perceived; and in eight days and twelve hours the liver was seen. In ten days and twelve hours the cutis was covered with cuticle, and the gizzard and intestinal canal were formed. The above, as well as several intermediate changes, are illustrated by drawings, and the author concludes the paper with some observations upon the circumstances in which the changes observed during the incubation of the egg, differ from those which occur in the ovum of the quadruped.

Some Observations on Corrosive Sublimate. By John Davy, *M.D. F.R.S.* Read June 6, 1822. [*Phil. Trans.* 1822, *p.* 357.]

It has sometimes been stated that corrosive sublimate suffers decomposition by exposure to light; but Dr. Davy found this not to be the case with the dry salt. Its aqueous solution, however, and especially its solution in proof spirit (the Liquor Hydr. Oxymur. of the Pharmacopœia), when exposed to sunshine, deposits a little calomel, and forms muriatic acid. The alcoholical and ethereal solutions suffer no such change; nor do the aqueous solutions, to which small quantities of muriatic acid and of muriate of ammonia have been added.

The author found corrosive sublimate soluble in water at 57°, in the proportion of 5·4 per cent. Alcohol at 60° dissolved half its weight, and ether about one third its weight. Heated with oil of turpentine, corrosive sublimate gives rise to the formation of muriatic acid and calomel, carbon is deposited, and a little artificial camphor produced; with other oils the changes are of a similar description.

Muriatic acid of sp. gr. 1·158 at 74°, dissolves twice its weight of corrosive sublimate, the specific gravity of the resulting solution being 2·412; when the temperature of this solution is somewhat lowered, it concretes into a mass of acicular crystals.

Nitric acid at the temperature of 90°, does not dissolve corrosive sublimate, nor does sulphuric acid.

A mixture of 34 parts of corrosive sublimate, and 6·75 of muriate

of ammonia, liquefies when heated, being more fusible and less volatile than the ingredients separately, and concretes into a gray crystalline mass on cooling. The author then describes the property of several solutions of corrosive sublimate and sal-ammoniac, showing that the solubility of the compound salt exceeds that of the most soluble ingredient; and details some experiments illustrating the action of the muriates of baryta, magnesia, potash, and soda, upon corrosive sublimate.

On the State of Water and Aëriform Matter in Cavities found in certain Crystals. By Sir Humphry Davy, *Bart. P.R.S.* Read June 13, 1822. [*Phil. Trans.* 1822, *p.* 367.]

After adverting to the interesting phenomena connected with certain crystalline products of the globe, and showing that the Huttonian theory more plausibly accounts for their formation than the Wernerian, Sir Humphry proceeds to offer additional arguments in its favour, deduced from his examination of the aëriform and liquid matter contained in certain siliceous stones. The fluid was in all cases found to be nearly pure water; and the elastic fluid was pure azote, existing always, however, in a state of considerable rarefaction; namely, from 12 to 18 times more rare than atmospheric air. In the only two cases in which the relation of the bulk of the water to that of the void space could be ascertained, it was nearly as 2 to 1.

In the chalcedonies of basaltic rocks the gas was also azote, but it was 61 or 70 times more rare than atmospheric air, the quantity of water to that of void space being the same as in the rock crystal.

It occurred to the author that atmospheric air might have been originally included, and that the oxygen might have been absorbed by the water; and an experiment is detailed, the result of which proved favourable to such an opinion. None of the crystals of secondary rocks examined by Sir Humphry Davy were impervious to air; in these, therefore, atmospheric air was found of its usual density; this was even the case with the cavities in dense calcareous spar.

The President observes, in conclusion, that it appears difficult to explain the results obtained, unless by supposing the water and silica separated from each other at a very high temperature; at such temperatures a liquid hydrate of silica might exist under pressure, and like other liquid bodies in the atmosphere, it would probably contain small quantities of atmospheric air; and upon such a supposition, the phenomena presented by the water in rock crystal and chalcedony might be accounted for.

Some Experiments on the Changes which take place in the fixed Principles of the Egg during Incubation. By William Prout, *M.D. F.R.S.* Read June 20, 1822. [*Phil. Trans.* 1822, *p.* 377.]

In the researches detailed in this paper, the author's attention was chiefly directed to the nature, proportions, and changes of the earthy and saline substances contained in the egg, and to the source of the matter constituting the skeleton of the chick. He therefore analysed the egg first in its recent state, and then at the end of the first, second, and third weeks of incubation, his experiments being principally confined to the eggs of the domestic fowl.

After some preliminary details relating to the variations in weight which eggs suffer when kept, and which show that for a considerable period they sustain a daily loss of about nine grains, and that the relative weights of the shell, albumen, and yolk, are liable to considerable variation, Dr. Prout proceeds to describe the manner in which he conducted his analysis, especially in relation to the saline principles of the yolk. This substance is remarkably difficult of incineration, in consequence of the phosphorus which it contains burning into phosphoric acid, which forms a coating that protects the coal from the action of the air. The general results of these experiments are thrown into the form of tables. They show that the relative weights of the constituent principles of different eggs vary considerably, and that during incubation the loss of weight exceeds by about eight times that which the egg sustains by ordinary keeping. That in the earlier stages of incubation an interchange of principle takes place between the yolk and a portion of the albumen, which passes into a substance in some respects analogous to curd of milk. That as incubation proceeds, the phosphorus of the yolk becomes phosphoric acid, which, united with lime, is found in the bones of the chicken; which lime, Dr. Prout thinks, makes its appearance in some unaccountable manner, and from some unknown source, and that it does not pre-exist in any known state in the recent egg. Its only possible source, observes Dr. Prout, is the shell; but we are precluded ascertaining the exact quantity of lime in any particular shell before and after incubation, and the application of averages cannot be resorted to, in consequence of the great difference of weight in the shells of different eggs. The author's reasons for doubting that origin, or source of the lime, are, that the membrane in contact with the shell is never vascular, and that both the albumen and the yolk contain at the end of incubation a considerable quantity of earthy matter, which one would suppose would have been appropriated to the bone in preference to that derived from a more remote source. Dr. Prout observes, that the circumstance of the shell becoming very brittle towards the end of incubation, may by some be ascribed to the absorption of a portion of its substance; but this he is inclined rather to refer to the heat requisite to the process of incubation, which he thinks is sufficient to account for all the known changes which the shell suffers. He is of opinion that great doubt hangs over

the source of the lime, though he by no means intends to assert the impossibility of its being derived from the shell.

On the Placenta. By Sir Everard Home, *Bart. V.P.R.S.* Read June 27, 1822. [*Phil. Trans.* 1822, *p.* 401.]

After adverting to the discovery of the unattached human ovum in the uterus, and to the formation of the placenta independent of the chorion, the author proceeds to show that the office of the placenta, and when that is wanting, of the chorion, being to supply materials for the growth of the embryo, it varies in structure, and has a peculiar form in every genus of animal, and is to be considered as the means employed by Nature to prevent the whole system respecting animals from being thrown into confusion by any two different genera continuing to interbreed with one another, and which cannot take place without a new form of placenta is employed, for which there is no provision. The author then states that the period of utero-gestation depends upon the structure of the placenta or chorion; that where they are very vascular it will be short, and where the reverse very long; and that where the female of one species breeds from the male of another, whose utero-gestation is different, the hybrid is brought forth at the end of the longest period of the utero-gestation of the species concerned. Sir Everard thinks that the placenta and chorion are instrumental in producing the stimulus, in consequence of which parturition comes on; for as soon as double circulation takes place in the fœtus, which is not till the lungs are perfected, the placenta is so much diminished, that this alone may produce a separation of the placenta and chorion, and expulsion of the young. The author concludes this paper with a specimen of a new classification of animals, founded upon the variation of structure of the placenta, of which the first class only is given, including animals in which the ovum becomes attached to the womb of the mother. This class comprises seven orders, distinguished by peculiarities in the structure of the chorion and placenta.

Of the Geographical Situation of the Three Presidencies, Calcutta, Madras, and Bombay, in the East Indies. By J. Goldingham, *Esq. F.R.S.* Read June 27, 1822. [*Phil. Trans.* 1822, *p.* 408.]

From an extended series of observations of the eclipses of Jupiter's satellites, corrected for the difference of the tables from the observations taken at Greenwich, at or about the time of each eclipse, the mean longitude of the Observatory at Madras is 80° 17′ 21″ E., and the mean latitude found by meridional observations of the sun and stars north and south of zenith, taken with the sextant, circular instrument, and zenith sector, is 13° 4′ 9″ N.

By a series of corresponding eclipses of Jupiter's satellites, taken at Fort William, the longitude of the Fort appears to be 88° 23′ 39″ E.

By a series of lunar observations, and of the eclipses of the satellites of Jupiter, the mean result duly corrected, gives the longitude of Bombay Church 72° 54′ 43″ E., and of the Lighthouse 72° 53′ 36″ E. of Greenwich. The mean latitude found by 32 meridional observations of the sun and stars, duly corrected, was found to be 18° 56′ 7″ N. for the Church, and 18° 54′ 25″ for the Lighthouse.

Of the Difference of Longitudes found by Chronometer, and by correspondent Eclipses of the Satellites of Jupiter; with some supplementary Information relative to Madras, Bombay, and Canton; as also the Latitude and Longitude of Point de Galle and the Friar's Hood. By J. Goldingham, *Esq. F.R.S.* Read June 27, 1822. [*Phil. Trans.* 1822, *p.* 431.]

The longitude of Masulipatam Flagstaff by the eclipses is 81° 12′ 33″, and by the chronometer 81° 12′ 15″, which is so close an agreement, that the longitude of this important point of the coast may be regarded as correctly determined. Mr. Goldingham then details the observations by which he determined the latitude and longitude of Point de Galle Flagstaff, the mean of the former being 6° 0′ 50″ N., and the latter 80° 17′ 2″ E.; the longitude of Canton, deduced as the mean of Capt. Huddart's observations and his own, Mr. Goldingham gives as 113° 18′ 23″ East of Greenwich.

Observations on the Genus Planaria. *By* J. R. Johnson, *M.D. F.R.S.* Read June 27, 1822. [*Phil. Trans.* 1822, *p.* 437.]

In this paper the author confines his observations to four species of the above genus, which are delineated of their natural size in an annexed drawing. They are generally clustered under leaves, stones, or pieces of wood, in slow streams; they are very rapid and changeable in their movements, in consequence of the annular muscles of which the body consists; their texture, however, is so soft, as not to admit of accurate dissection, though the author was able to ascertain that the body consisted of one common cavity, with lateral cells, like that of the medicinal leach. In the *Planaria torva*, two ventral apertures are particularly distinct. The upper one gives passage to a long flexible tube, and the lower conducts to the ovarium; this tube they frequently project, and employ it in seizing worms and aquatic insects; they also receive their food by this organ, and not exclusively by the mouth, as the author proved by presenting an earth worm to one of the *Planariæ lacteæ*, from which he had removed the head; it soon affixed itself, and became distended by food. When, however, the animal is injured, or loses this tube, it then takes sustenance by the mouth. Though he has repeatedly seen the young of the *Planaria torva* and *Planaria cornuta*, the author has not determined whether they are oviparous or viviparous. The *Planaria lactea* and *Planaria brunnea* are oviparous, producing eggs within a

membranous capsule, each of which produces from 3 to 8 young, which, on escaping from the egg, are of various sizes, and very active.

But the most singular part of the history of these animals is, that they not only perpetuate their species as above described, but also by a natural division of their body into two portions, the head part reproducing a tail, and the tail a head in about fourteen days. These appearances are represented in annexed drawings, and several experiments are detailed in further illustration of their reproduction, showing that a perfect animal is producible.

Some Experiments and Researches on the Saline Contents of Sea-water, undertaken with a view to correct and improve its Chemical Analysis. By Alexander Marcet, *M.D. F.R.S. Honorary Professor of Chemistry at Geneva.* Read June 27, 1822. [*Phil. Trans.* 1822, *p.* 448.]

At the commencement of this paper Dr. Marcet, after adverting to the conclusions at which he arrived in some former researches communicated to this Society, notices the extraordinary assertions of Rouelle and of Proust respecting the existence of mercury in sea water. By a very careful examination, however, of bay salt, he was unable to detect the smallest trace of that metal; nor did he find it in a sample of Sel de Gabelle obtained from Calais for the purpose of examination.

Dr. Marcet next examined sea water, with a view of ascertaining whether any nitrates are present in it: with this view he added sulphuric acid and gold leaf to the concentrated bittern, and boiled the mixture, but the metal was not in the least acted on; when, however, the smallest quantity of nitre was added, the gold was instantly dissolved. Hence the absence of nitric salts in sea water may be inferred.

In examining some of the same bittern for earthy and metallic salts, the author found that neither alkalies nor their carbonates throw down anything but magnesia, and that no muriate of lime appears in any case to be present. Selenite and carbonate of lime were, however, found in the matters deposited during the first evaporation of the transparent and pure sea water.

When sea water is evaporated to dryness, and the residue submitted to distillation at a red heat, Dr. Marcet found that a portion of sal-ammoniac sublimed. Lastly, he observes, that no sulphate of soda is discoverable in sea water, but that it affords, on evaporation along with other salts, certain rhombic crystals, which are triple sulphate of potash and magnesia.

On the Ultimate Analysis of Vegetable and Animal Substances. By Andrew Ure, *M.D. F.R.S.* Read June 27, 1822. [*Phil. Trans.* 1822, *p.* 457.]

Dr. Ure commences this paper by adverting to the fallacies to which the modes of analysing organic substances hitherto practised are subject; and in detailing the peculiar methods adopted in his own researches, he shows the means of obviating them, and of diminishing the various sources of inaccuracy to which these complicated processes of analytical chemistry are necessarily more or less liable. Where oxide of copper is used, its hygrometric quality has generally been overlooked, or not duly allowed for; and the animal and vegetable substances have not in general been exposed to any process of desiccation sufficiently exact or uniform; the author therefore always used the oxide of copper in some known or ascertained degree of humidity; and he dried the organic bodies in the air-pump vacuum, aided by the absorbent powers of a surface of sulphuric acid in the apparatus, and with precautions which he fully describes. He then details the best means of applying heat for the decomposition of organic substances, and describes a drawing representing the construction of his furnace, and other implements. Lastly, he points out the method of examining the results and products, and gives in detail the analysis of sulphuric ether, as illustrating the mode of computing the relations of the constituents, while the results of the other analyses are, for the sake of brevity, thrown into a tabular form. Dr. Ure concludes his paper with some general remarks on the analytical details. In respect to sugar, he observes, that on comparing pure crystalline sugar with diabetic sugar, the latter exhibits a notable excess of oxygen; and he considers weak sugars (as the refiners call them), in general, to exhibit the same peculiarity.

In applying the atomic theory to his experimental results, the author enlarges on the different views which may be taken of the ultimate constitution of a variety of organic products, and enters at considerable length into details relating to the vegetable acids, with a view of determining with exactness their prime equivalents, and the relative proportions of combined water which they contain in their crystalline states.

The Croonian Lecture. Microscopical Observations on the Suspension of the Muscular Motions of the Vibrio tritici. *By* Francis Bauer, *Esq. F.R.S. F.L.S. and H.S.* Read December 5, 1822. [*Phil. Trans.* 1823, *p.* 1.]

The *Vibrio tritici* is a small worm which infects wheat, being the immediate cause of that destructive disease called Ear Cockle, or Purples. Upon examining the grains thus diseased, the author found them to be the unimpregnated germens, containing masses of a white and apparently gluey mucus, which might be removed in the shape of a firm ball, and which, when immersed in water, and viewed

through the microscope, displayed hundreds of minute worms in lively motion. When these worms had become perfectly dry, and apparently entirely lifeless, they again recovered upon being moistened with a drop of water, and were as lively as before.

To determine the origin of these animals, Mr. Bauer undertook a series of experiments, which convinced him that the spawn or eggs were conveyed into the cavities of the germens by the circulating sap. In these experiments he inserted some of the worms into sound grains of wheat, suffered them to germinate, and found the worms in different stages of their growth in the stalk, and ultimately in the germens.

The largest of these worms was one fourth of an inch long, and one eightieth of an inch in diameter; their head is armed with a moveable proboscis, and the tail ends in a claw-like point; at a small distance from which, on the inferior side, is an orifice, from which they discharge their eggs in strings of five or six, adhering to each other. Each egg is about [illegible]th of an inch long, and [illegible]th, or [illegible]th in diameter; and if attentively examined, they are transparent enough to allow of the young worm being seen within, which, in about an hour and a half after the egg is laid, extricates itself. These worms exhibit no external distinctions of sex, and the author considers them to be hermaphrodites.

The first specimens of these worms which Mr. Bauer examined, were from grains twelve months old, and consequently perfectly dry. He, however, also succeeded in recovering them by immersion in water, from wheat which had been kept five years and eight months; but the longer the specimens were kept, the longer were the worms obliged to be immersed in water, to enable them to recover their muscular motions. The longest period of its suspension which he had observed, was six years and one month; after that time they seemed perfectly dead.

Alternately moistened and dried in a watch-glass, these worms might be preserved alive for several weeks; and if kept continually moist, they remained alive for three months; but if dried at the end of that period, they do not again recover, but become quite straight, and remain unaltered in the water for more than fourteen months, when they gradually decay. Their extraordinary preservation, and these various circumstances, Mr. Bauer refers to the mucous-like water in which they are enveloped, and which appears to be of an oily nature.

The author concludes this paper with an abstract of the description of these worms given by other writers, and of their opinions respecting their origin.

On Metallic Titanium. By W. H. Wollaston, *M.D. V.P.R.S.* Read December 12, 1822. [*Phil. Trans.* 1823, *p.* 17.]

Small cubic crystals are occasionally met with in the slag of iron furnaces, which, from being imbedded in sulphuret of iron, have been

mistaken for pyrites. Upon subjecting them, however, to more rigid examination than they had previously received, Dr. Wollaston ascertained them to be titanium in its metallic state. He found them not only harder than pyrites, but so hard as to scratch glass, and even agate. They are neither acted upon by nitric, sulphuric, nor muriatic acids; nor are they dissolved by nitro-muriatic acid. They are infusible before the blowpipe, but become superficially oxidized, and borax restores the cleanliness of their surface, by dissolving the oxide. By nitre they are rapidly oxidized; and by combining its action with that of borax, they may be entirely dissolved. The fused mass is soluble in muriatic acid; and from this solution the alkalies precipitate a white oxide, insoluble in pure and carbonated alkalies. When evaporated, the excess of muriatic acid may be driven off, and a soluble muriate remains, in a state favourable for exhibiting the leading properties of titanium. Infusion of galls produces in this solution the characteristic red precipitate; prussiate of potash occasions one of similar colour, which differs from prussiate of copper by inclining to orange instead of purple, while prussiate of uranium is rather brown than red.

Although the crystals are imbedded in sulphuret of iron, Dr. Wollaston found in them neither iron nor sulphur. That they are in the metallic state, is proved by the perfection with which they conduct a feeble degree of electricity. They did not unite with tin, lead, silver, or copper. From their extreme infusibility, Dr. Wollaston thinks that they have not been formed by crystallization in cooling from a state of fusion, but have received their successive increments by reduction of the oxide dissolved in the slag around them,—a mode of formation to which we must have recourse for conceiving rightly the formation in nature of many other metallic crystals.

On the Difference of Structure between the Human Membrana Tympani and that of the Elephant. By Sir Everard Home, *Bart. V.P.R.S.* Read December 12, 1822. [*Phil. Trans.* 1823, *p.* 23.]

In an elephant three weeks old, the membrana tympani was of an oval form, 1½ inch long, and 1⅛ broad. The muscular fibres lie upon its inner surface, and terminate by an attachment to the point and two sides of the malleus, so that one portion of the fibres is short, and the other more than double their length. From this structure the elephant cannot adapt its ear to musical sounds in the same manner the human ear does; but in Sir Everard's opinion, it is enabled by the long fibres to hear sounds at a great distance. In regard to musical sounds, high notes scarcely excite its attention, but it listens to the lower ones with apparent satisfaction. In neat cattle, and in the deer, the membrana tympani is oval, and the structure approximates to that in the elephant. In the horse, the hare, and the cat, the handle of the malleus lies in the middle line, so that the fibres on the two sides are equal, and the organ appears similarly constructed in the whole of the feline tribe.

Corrections applied to the Great Meridional Arc, extending from Latitude 8° 9′ 38″·39 to Latitude 18° 3′ 23″·64, to reduce it to the Parliamentary Standard. By Lieutenant Colonel W. Lambton, *F.R.S. and Corresponding Member of the Royal Academy of Sciences at Paris.* Read January 9, 1823. [*Phil. Trans.* 1823, *p.* 27.]

It appears from the investigations detailed in this paper, that with respect to a measurement on the meridian, the degree depending on Colonel Lambton's brass scale must be multiplied by the fraction ·000018, and the product subtracted from the measure given by the scale to reduce it to the present parliamentary standard; and that the degree depending on Ramsden's bar must be multiplied by ·00007, and the product added to the measure given by the scale to reduce it to the standard.

On the Changes which have taken place in the Declination of some of the principal fixed Stars. By John Pond, *Esq. Astronomer Royal, F.R.S.* Read April 18, 1822. [*Phil. Trans.* 1823, *p.* 34.]

The objects of this communication are chiefly two; the first is to restore a greater degree of confidence in the results of the late observations made with the mural circle at Greenwich, which now appear to have been subjected to a very small error only, arising from some temporary causes now very effectually removed; and the second to point out a want of uniformity in the proper motions of almost all the stars, which is of such a nature as to indicate a slow change of place towards the south in almost every instance, with the exception of γ Ursæ majoris, β Ursæ minoris, and β Cephei only. The greatest deviation is found in three very bright stars, Capella, Procyon, and Sirius. The proper motion of each of these is southward; hence these proper motions are accelerated, while that of Arcturus, on the contrary, may be considered as uniform.

The author observes, that though the number of stars which have proper motions northwards is nearly equal to those of which the proper motion is southward, yet the joint magnitude of the motions southward exceeds that of the motions directed northwards nearly in the proportion of 4 to 1.

It was not till after February, 1821, that the mural circle became completely out of repair; its present perfection has been verified by means of observations made with an artificial horizon of mercury protected by wooden boxes, of different sizes and figures according to the different altitudes of the stars: at the same time, Mr. Pond observes, that for very delicate purposes it would be improper to place implicit confidence in the observations of declination made in the course of the year 1820.

Appendix to the preceding Paper on the Changes which appear to have taken place in the Declination of some of the fixed Stars. By John Pond, *Esq. Astronomer Royal, F.R.S.* Read November 14, 1822. [*Phil. Trans.* 1823, *p.* 39.]

The author states that the observations made at Greenwich during the last summer have confirmed his former results, showing most decidedly that the computed places of the principal fixed stars do not agree with those determined by actual observation. He finds the general tendency of the deviation to be southward, but inconsiderable in about one third part of the heavens as to right ascension, in which part, from the zenith to the pole, stars appear a very small quantity to the northward; in the remaining part, as to right ascension, all the stars deviate to the south, and with a few exceptions the southern more so than the northern ones.

He then inquires, whether these deviations admit of explanation by supposing them to arise from errors in the instrument, or in the mode of observing. If occasioned by the general uncertainty of astronomical observation, they would be found to follow no law at all; if by errors in the instrument, they would bear some direct proportion to zenith distance. But these anomalies are shown to depend rather on right ascension than on zenith distance; and as it cannot be conceived in what way error of zenith distance can be influenced by right ascension, the supposition of error in the instrument will not account for the deviations. In further proof of this, numerous examples are given of unequal or opposite deviation in stars adjacent as to polar distance, but of opposite right ascension; in all which cases, owing to the shortness of the arc to be measured, and the constancy therefore of the error affecting it, the deviation of the stars, if owing to the instrument, would be the same both in direction and degree.

The author next considers the different hypotheses that may be entertained by those who, not convinced by the preceding arguments, may yet be disposed to attribute these discordances to error. Admitting the accuracy of Bradley's observations to form the groundwork of the present inquiry, they must suppose either the Catalogue of 1813, or that of the present period, to be erroneous. If, assuming the present catalogue to be correct, we interpolate between it and that of Dr. Bradley, for the purpose of ascertaining the defects of the Catalogue of 1813, the latter will be found to have been erroneous beyond belief, and that of Dr. Brinkley, made about the same period, to have been rather more so. It will also follow, from the same admission, that the errors attributable to flexure committed in 1813 have since disappeared, and the instrument has gradually become more perfect; whereas the natural tendency of such a defect would be to increase, and to give receipts every year more and more distant from the truth.

The validity of the second hypothesis is next examined, which, assuming the Catalogue of 1813 to be correct, regards the present

as erroneous; and this leads the author to inquire, what confidence is due to the Greenwich observations of the present day. His principal reliance on their accuracy he founds on the coincidence of the results obtained by the two independent methods of direct vision and reflection, and he shows in what manner this coincidence of the two results is a proof of the accuracy of either. The general tendency of all instruments, he observes, is to undergo equal and opposite flexures, at equal elevations on either side of the zenith; whence it cannot be inferred, with regard to instruments that turn in azimuth, that flexure does not take place, from stars of different altitudes giving the same error of collimation. But on the contrary, if, by the method of reflection, stars of different altitudes give the same horizontal point, the strongest proof will be afforded of the non-flexure of the instrument. These equal and opposite flexures on opposite sides of the zenith, will, in our latitudes, affect stars near the equator by errors in polar distance about double of the errors committed in stars near the zenith. These views are illustrated by a comparison of the Greenwich Catalogue with those of Dr. Brinkley and Mr. Bessel; and since the two latter, in conformity with the law of flexure, differ from one another by 5″ near the equator, and by only half that quantity near the zenith, it is inferred that the instruments used by those two astronomers, one or both of them, are liable to flexure.

Since those stars must be most correctly determined by the method of reflection, whose altitude is the least, on account of the shortness of the arc to be measured, the author considers himself intitled to decide what is the share of error which, in stars near the equator, belong to each of these catalogues. In stars so situated, Dr. Brinkley's polar distances exceed the author's by about 2″, and Mr. Bessel's fall short of them by about 3″; and since the Greenwich Catalogue everywhere divides the difference of the two catalogues in nearly the same ratio, it is considered probable that the errors of each throughout are nearly in the same proportion.

On the Parallax of α Lyræ. By John Pond, *Esq. Astronomer Royal, F.R.S.* Read November 14, 1822. [*Phil. Trans.* 1823, *p.* 53.]

The author, not satisfied with his former attempts to discover the parallax of *α* Lyræ with the mural circle, has resumed his examinations of this star with the same instrument, being prevented from employing the method of a fixed telescope, for want of an opposite star sufficiently bright to be observed throughout the year. The Dublin observations having indicated that the parallax of *γ* Draconis is insensible, and that of *α* Lyræ about 2″, he investigates first the difference of parallax between these stars; secondly, the absolute parallax of *α* Lyræ.

The change in the angular distance of the two stars at opposite seasons, he finds not to exceed one tenth of a second; which perfect coincidence he attributes in great measure to the mildness of the winter of 1821-1822, enabling him to equalize the temperatures of

the outer and inner thermometer throughout the whole course of observation. It is shown that temperature cannot have vitiated these results by its effect on the instrument, since they are the same whether we employ the readings of two, or of six microscopes.

The absolute parallax of α Lyræ is investigated by the method of reflection, which the author had successfully introduced into other astronomical researches. Although the period of observation embraces only half the period of the double parallax, yet that apparent disadvantage is more than compensated, in the author's opinion, by an uniformity of the temperature obtained, such as can never be expected between the extreme seasons. Here, again, from the season chosen, from the pains taken to equalize the temperature, and from the concordance of the results obtained with two and six microscopes, the author believes that no errors of sensible amount have been introduced by change of temperature. These observations indicate that the absolute parallax of α Lyræ does not exceed a small fraction of a second.

The argument that has been advanced by Dr. Brinkley in favour of parallax, and on which the greatest reliance has been placed, is next adverted to; namely, that founded on the disengagement of the solar nutation after allowing for parallax, from the observations made with the Dublin instrument. This reasoning is considered strictly logical, as proving the disengagement of two equations, having each a regular period; but by no means so, as establishing that the larger equation results from parallax; since those stars in which the Dublin instrument discovers parallax, are at some distance from the zenith; and the more so as their parallax appears to be greater; and moreover, since those in which the greatest parallax is found, are stars whose maximum and mininum of parallax fall in the extreme seasons; the author thinks it probable that the discordances observed are owing to changes of temperature, which either alter the form of the instrument, or modify the refractions of the ray introduced within the observatory; since, on this supposition, we shall account for the want of parallax in zenith stars, and in those whose greatest and least parallax would happen at the mean seasons, and also for the regularity of the period that the discordances have been found to observe.

Observations on the Heights of Places in the Trigonometrical Survey of Great Britain, and upon the Latitude of Arbury Hill. By B. Bevan, *Esq. Communicated by Sir* H. Davy, *Bart. P.R.S.* Read May 23, 1822. [*Phil. Trans.* 1823, *p.* 73.]

The result of the trigonometrical survey, relative to the different sections of the meridian in this country, having disappointed public expectation, Mr. Bevan lately examined with some care the calculations affected by the observations made at Arbury Hill, with the hope of reconciling the anomaly in that part of the meridian. The

height of this station he determined by levelling to the Grand Junction Canal, from which, and the known difference of levels of the canals communicating with this, he obtained the relative height of this station, compared with the most important objects in Northampton, Buckingham, and Bedford. Finding the country to the north of Arbury station suddenly fall about 400 feet, and continue thus depressed for nine or ten miles, Mr. Bevan observes that such a defect of matter would probably produce a deflection of the plumb-line to the southward; and accordingly, on calculating the latitude of Arbury station from that of Blenheim observatory, independent of any astronomical observations made at Arbury, he found it 5″ less than shown by the zenith sector, giving countenance to the probability of local attraction by the high land to the south of the station. The author thinks that the observations at Dunnose were affected by the high land to the north of that station giving a latitude less than it should be by 7″ or 8″, and that Greenwich observatory is not altogether clear of local attraction from the higher land to the south, and defect upon the northern side. Clifton station also, he remarks, may be 2″ or 3″ in error, from the same cause.

With such corrections as the face of the country may warrant, not exceeding in the whole 200 miles above 10″, the author thinks it probable that the section of the meridian measured in Britain may agree with the different sections measured in other parts of the world.

Mr. Bevan lastly adverts to the probable errors in the *height* of the stations in the survey; and assuming the highest points of the Grand Junction Canal to be 408½ feet above the level of the sea at low-water spring tides, he considers the heights of Wendover, Kensworth, Bowbrick Hill, and Arbury Hill stations, to be about 72 feet in excess, as laid down in the survey.

On some Fossil Bones discovered in Caverns in the Limestone Quarries of Oreston. By Joseph Whidbey, *Esq. F.R.S. In a Letter addressed to* John Barrow, *Esq. F.R.S. To which is added, a Description of the Bones by Mr.* William Clift, *Conservator of the Museum of the College of Surgeons.* Read February 6, 1823. [*Phil. Trans.* 1823, *p.* 78.]

In one of these caverns there was a lining of stalactite, and the bones were lying loosely covered with rubble; in another, the bones adhered to the walls.

To this letter is annexed a description of the bones found by Mr. Whidbey, by Mr. Clift, Conservator of the Museum of the Royal College of Surgeons.

They belong to animals of several distinct genera; namely, the Bos, the Deer, Hyæna, Horse, Wolf, and Fox. Of these bones, a few are superficially incrusted with stalagmite, but the greater number were firmly imbedded in stiff clay, and exhibit no appearances of

mutilation, except in one instance, where the radius of a young wolf is impressed by the incisors and canine teeth of an animal the size of the weasel.

Such of the bones as were examined appeared to have lost the greater part of their animal matter, and had consequently become brittle; some of them when immersed in water became black, but recovered their former appearance on drying; this was especially the case with those of the carnivorous tribes.

Mr. Clift observes that appearances of disease in fossil bones are of rare occurrence; among these, however, he found two examples in the metacarpal and metatarsal bones of the bovine animals, showing upon their surface the effect of ossific inflammation; there were also marks of disease in the lower jaw of a young wolf.

It appears from Mr. Clift's detailed enumeration of the bones from these caverns, that they are clearly referable to animals of known and still existing genera; but he observes that it is a curious circumstance, that with the exception of the very few belonging to the deer, they all appertain to animals differing from those formerly found in the immediate vicinity of the present caverns.

Mr. Clift concludes this communication with a particular description and enumeration of the bones, which are further illustrated by reference to several drawings.

On the Chinese Year. By J. F. Davis, *Esq. F.R.S.* Read December 19, 1822. [*Phil. Trans.* 1823, *p.* 91.]

After stating his opinion that the Chinese are possessed of no original astronomical knowledge, but that that which they possess is entirely of foreign origin, since in former times they even adopted the errors of European astronomers; and that the instruments mentioned by Du Halde as having been found by the missionaries on their first entrance into the country, were constructed by the Arabians; the author proceeds to confirm this opinion by an account of the division of the Chinese year, and a comparison of the Chinese with the European zodiac. The former is divided into twenty-eight constellations, and Mr. Davis has represented these in an annexed drawing, with the number of degrees affixed to each; from which it appears that they are extremely unequal, the largest consisting of 30°, and the least of not more than 5°. Of these constellations, Kio, which corresponds to a part of Virgo, is considered as the first in order; which is perhaps a proof, says the author, that in some former period their year commenced at this point. As far, however, as Mr. Davis's information, the Chinese have no solar year, their year, properly considered, being a lunar year, consisting of twelve months, of twenty-nine and thirty days alternately, with the occasional addition of a thirteenth month, to make it correspond more nearly with the sun's course.

Experiments for ascertaining the Velocity of Sound, at Madras in the East Indies. *By* John Goldingham, *Esq. F.R.S.* Read February 20, 1823. [*Phil. Trans.* 1823, *p.* 96.]

The author commences this paper with an abstract of the opinions, experiments, and calculations of several eminent philosophers who have studied the above subject; and after remarking upon their discordant results, observes that his own experiments may perhaps furnish a clue for discovering the cause of such differences. These were made at Fort St. George, where a morning and evening gun are fired from the ramparts, the former at daylight, and the latter at 8 o'clock in the evening. Morning and evening guns are also fired at St. Thomas's Mount; and between these the Madras Observatory is situated, in latitude 13° 4′ 8″ N., at which the observations were made with chronometers of 100 beats in 40 seconds. Each observer began to count the beats in the interval of the flash and report. Their number was registered, as well as the height of the thermometer, barometer, and hygrometer, and the state of the wind and weather at the time. The distances of the guns from the observatory were ascertained with much precision; that of the Mount gun being 29547 feet, and of the Fort gun 13932·3 feet. The results of these experiments are given in eleven annexed tables. From Tables 1 and 6, it appears that the velocity of sound is much affected by different states of the atmosphere and weather; and from Tables 2 and 7, we find as the thermometer rose, the atmosphere at the same time decreasing in density, sound moved with increased rapidity. The mean velocity of sound deduced from these experiments appears to be 1142 feet in a second, which closely corresponds with the estimate of Newton and Halley. The comparison of the experiments with the Mount and Fort guns seems to show that sound travels equally in its progress; and by comparing the observations upon the influence of wind, a difference is found of 1275 feet in a minute between the wind being in the direction of the motion of the sound, and opposed to it.

On the Double Organs of Generation of the Lamprey, the Conger Eel, the common Eel, the Barnacle, and Earth Worm, which impregnate themselves; though the last from copulating, appear mutually to impregnate one another. *By Sir* Everard Home, *Bart. V.P.R.S.* Read February 27, 1823. [*Phil. Trans.* 1823, *p.* 140.]

Having previously ascertained the teredo and the lamprey to be hermaphrodites, and that in these tribes the same individual both forms and impregnates the ova, the author shows upon the present occasion, that eels and barnacles are similar in their mode of generation. In respect to the former, the author adopts the opinion of Sir Humphry Davy, that the common and the conger eel belong to the same species, their difference in size and colour depending upon the one living in fresh and the other in salt water. Their organs of

eneration are precisely similar. Sir Everard points out some errors nto which anatomists had fallen in describing these, more especially with respect to the eel tribe, in which the kidneys being immediately behind the peritoneum, and closely connected, the whole mass has een mistaken for kidney.

In that species of barnacle called *Lepas anatifera*, the ovaria are situated round the œsophagus, and the ova are impregnated before hey leave the ovaria.

The author concludes this paper with an account of the structure f the organs of generation in the earth worm, and of their mode of opulation. These, as well as the other anatomical facts detailed in his communication, are illustrated by reference to a variety of drawngs.

On a New Phenomenon of Electro-magnetism. By Sir Humphry Davy, *Bart. Pres. R.S.* Read March 6, 1823. [*Phil. Trans.* 1823, *p.* 153.]

About fifteen months ago it occurred to Sir Humphry Davy to try he action of a magnet upon mercury, connected in the electric circuit; and having very lately had occasion to repeat the experiment n a more perfect manner, by the aid of a battery, consisting of a single pair of plates of about 100 square feet, constructed for the London Institution, under the direction of Mr. Pepys, he is induced o lay the result of the experiment before the Royal Society, as presenting a phenomenon which may prove important hereafter in its relations to the theory of electro-magnetism.

When two wires were placed in a basin of mercury, perpendicular to the surface, and in the voltaic circuit of the above-mentioned battery, the mercury revolved according to the common law of electro-magnetic rotation, upon presenting a magnet either above or below the wires; and the velocity was increased by using the opposite poles of two magnets, one above and the other below the mercury. When the pole of the magnet was held above the mercury, and between the two wires, the circular motion ceased, and currents ook place in the mercury in opposite directions. These and other circumstances induced Sir Humphry Davy to believe that the passage of the electricity through the mercury, produced motions independent of the magnet, and that the rotations described were owing o a composition of forces; and, moreover, that such motions would, from the position of the wires, occur chiefly at the lower surface of he mercury; he therefore inverted the form of the experiment, bringing the copper wires through two holes in the bottom of a glass basin, with so much mercury in it as to stand one tenth of an inch above the polished ends of the wires. Upon making the communication with the battery, the surface of the mercury was elevated into a small cone above each of the wires, from which waves flowed off in all directions, the only apparent point of rest being central, between the wires. These cones were diminished by the approximation of

the pole of a magnet, which produced rotation, and on bringing it near enough, a depression of the mercury above the pole. The above phenomenon appeared, independent of any elevation in the temperature of the mercury, nor can it be attributed to electric repulsion. It must be referred to forces producing motions in right lines, or undulations from the surfaces of the wires as a centre; and it seems, says the author, strongly opposed to the idea of the electro-magnetic results, being produced by the motion of a single imponderable fluid.

On Fluid Chlorine. By M. Faraday, *Chemical Assistant in the Royal Institution. Communicated by Sir* Humphry Davy, *Bart. Pres. R.S.* Read March 13, 1823. [*Phil. Trans.* 1823, *p.* 160.]

By exposing the solid hydrate of chlorine, hermetically sealed up in a glass tube, to a temperature of about 100, the chlorine is evolved from it under such pressure that it assumes the liquid form, appearing of a bright yellow colour, and sinking in the warm water without showing any tendency to mix with it till the temperature fell to about 70°, when the whole re-assumed the appearance of solid hydrate. The liquid chlorine, in its pure form, did not congeal at 0°, and it instantly assumed its usual elastic form upon removing the pressure to which it was subjected. By condensing dry chlorine by means of a syringe into a glass tube, Mr. Faraday succeeded in converting a portion of it into a liquid, under a pressure of about four atmospheres.

The specific gravity of liquid chlorine he considers to be about 1·33.

In a note attached to this paper Sir Humphry Davy announces his having succeeded in obtaining muriatic acid in a liquid form, by causing sulphuric acid and muriate of ammonia to act upon each other in a strong sealed tube. The gas thus gradually liberated under pressure, condensed into an orange-coloured liquid, lighter than sulphuric acid, and instantly assuming the elastic state when the tube is broken.

Sir Humphry suggests the probability of other gases being condensed into the liquid form by a similar method of condensation under pressure; and points out the advantages which this mode possesses over a sudden mechanical pressure, and condensation by exposure to cold.

On the Motions of the Eye, in illustration of the Uses of the Muscles and Nerves of the Orbit. By Charles Bell, *Esq. Communicated by Sir* Humphry Davy, *Bart. P.R.S.* Read March 20, 1823. [*Phil. Trans.* 1823, *p.* 166.]

The author of this paper has entered into an examination of the external apparatus and muscles of the eye, with the view of explaining the necessity of six nerves being given to the parts contained in the orbit.

In the course of this examination he shows that the six muscles, rhich are attached to the eye-ball, do not, as has been supposed, orm one class of voluntary muscles; but that while the four straight nuscles, or recti, are provided for the voluntary motions of the eye rhen directed to objects, the other two, called oblique, perform ertain involuntary motions. These involuntary actions are shown o be a provision for the better protection of the eye; for when the ye-lids wink and close to wash the cornea, the effect would be incomplete, and the object but imperfectly attained, unless the cornea rere at the same time raised by the revolving of the eye-ball.

After having proved that the eye-ball revolves so as to carry the ornea upwards during the motion of the eye-lids; and having hown also that the oblique muscles are the agents in this involunary and instinctive motion,—he proceeds to demonstrate that the ame muscles elevate the cornea during sleep.

The author says, that while we are awake, the eye is under the ctive influence of the four straight muscles; but when the eye-lids re closed in sleep these muscles resign their office, and the involunary oblique muscles prevail, so as to draw the cornea under the ipper eye-lid. This is also shown to be the condition of the eye in aintness and on the approach of death, and on all other occasions rhen languor or debility prevail over the voluntary muscles of the rame.

The author notices, incidentally, that the enjoyment of the sense f vision is attended with the excited condition of the recti or vountary muscles, and that insensibility to the impression on the eye s followed by relaxation and neglect of the same class of muscles, nd consequently that a depraved and injured condition of the retina s one cause of squinting; for the oblique prevailing, while the recti r involuntary muscles are neglected, draw the eye so affected from he parallel line of vision.

After having shown that the recti, or voluntary muscles, are trictly associated with the activity of the retina or organ of vision, ie proceeds to express his opinion that the ideas received through the ye are not limited to the office of the retina, but that the sense of ision properly so called, is aided by the sense of voluntary exertion n the recti muscles, and afford us the knowledge of the position and elation of bodies, in addition to the ideas of form, shades, and coours, which are received through the retina.

The paper is illustrated by references to comparative anatomy, and y observations and experiments on man and brutes.

After having described the variety of actions performed by the muscles of the eye, the author proposes in the second part of the paper to arrange the nerves which go into the orbit according to their offices.

An Account of an Apparatus on a peculiar Construction for performing Electro-magnetic Experiments. *By* W. H. Pepys, *Esq. F.R.S.* Read April 10, 1823. [*Phil. Trans.* 1823, *p.* 187.]

This instrument consists of two plates, each fifty feet long and two wide, one of copper and the other of zinc. They are rolled round a cylinder of wood, and their contact is prevented by the interposition of hair rope and notched sticks. This coil is counterpoised and suspended by a rope and pulleys over a tub of acid, into which it is immersed when required for use. When thus immersed, with its poles connected by a sufficient conductor, it affected magnetic needles at a distance of five feet, and was extremely powerful in conferring magnetism upon steel bars, which acquire a north polarity at the copper, and a south at the zinc plate. The electric intensity of the apparatus is exceedingly feeble.

On the Condensation of several Gases into Liquids. *By Mr.* Faraday, *Chemical Assistant in the Royal Institution.* *Communicated by Sir* Humphry Davy, *Bart. P.R.S.* Read April 10, 1823. [*Phil. Trans.* 1823, *p.* 189.]

The gases which the author has succeeded in condensing into the liquid form, are, the sulphurous acid, sulphuretted hydrogen, carbonic acid, euchlorine, nitrous oxide, cyanogen, ammonia, muriatic acid, and chlorine. The process by which they were condensed, consisted in liberating them from certain of their compounds in small glass tubes, hermetically sealed and bent, so that when required, the end might answer the purpose of a receiver, and be occasionally immersed in ice or freezing mixtures. They generally appear as exceedingly limpid, colourless, and mobile fluids, and assume the gaseous form with various degrees of rapidity and violence upon the removal of that pressure by which they had been previously restrained.

In this paper Mr. Faraday details the particular method to which he resorted for obtaining each of these liquid bodies, and describes such of their characters as his experiments have hitherto enabled him to determine.

Liquid sulphurous acid appears to exert a pressure of about 2 atmospheres, at 45°. The pressure of the vapour of sulphuretted hydrogen was equal to about 13 atmospheres, at 32°; that of carbonic acid to 40 atmospheres, at 45°; of nitrous oxide 48 atmospheres, at 50°; of cyanogen between 3 and 4 atmospheres, at 45°; of muriatic acid 28 atmospheres, at 32°.

The author's attempts to obtain hydrogen, oxygen, fluoric, fluosilicic, and phosphuretted hydrogen gases in the form of liquids, have hitherto been without success.

On the Application of Liquids formed by the Condensation of Gases as Mechanical Agents. By Sir Humphry Davy, *Bart. P.R.S.* Read April 17, 1823. [*Phil. Trans.* 1823, *p.* 199.]

The elasticity of vapours, in contact with the liquids from which they are produced under high pressures in high temperatures, is known to increase in a higher ratio than the arithmetical one of the temperature; but the exact law is not determined, and the loss of latent heat in compression, and the re-absorption in expansion, renders the advantage of steam under great pressure and at very high temperatures doubtful in an economical view. No such doubt, however, exists in regard to those fluids which require very great compression for their existence, and where common temperatures are sufficient to produce an immense elastic force. Thus sulphuretted hydrogen, which condenses into a liquid under a pressure of 14 atmospheres at 3°, had its elastic force increased so as to equal a pressure of 17 atmospheres by raising its temperature to 47°. Liquid muriatic acid at 3° exerted an elastic force equal to the pressure of 20 atmospheres, at 25° = 25 atmospheres, and at 51° = 45 atmospheres.

After some experimental illustrations of the expansibility of the vapour of sulphuret of carbon at different temperatures, the author adverts to the possible application of the difficultly compressible gases, as mechanical agents, and to their power of producing cold by the rapidity of their evaporation.

On the Temperature at considerable Depths of the Caribbean Sea. By Captain Edward Sabine, *F.R.S. In a Letter addressed to Sir* Humphry Davy, *Bart. P.R.S.* Read April 17, 1823. [*Phil. Trans.* 1823, *p.* 206.]

In this letter Captain Sabine details the results of some experiments on the temperature of the Caribbean Sea, in lat. 20½° N. and long. 83½° W. At the depth of 1230 fathoms by the line, or about 1000 fathoms actual depth, a six's register thermometer indicated 45°·5, the temperature of the water at the surface varying from 82°·5 to 83°·2, so that the difference amounted to 37°·3.

Letter from Captain Basil Hall, *R.N. to Captain* Kater, *communicating the Details of Experiments made by him and Mr.* Henry Foster, *with an Invariable Pendulum, in London; at the Galapagos Islands in the Pacific Ocean, near the Equator; at San Blas de California on the N.W. Coast of Mexico; and at Rio de Janeiro in Brazil. With an Appendix, containing the Second Series of Experiments in London, on the Return.* Read April 24, 1823. [*Phil. Trans.* 1823, *p.* 211.]

Captain Hall stated that the ship which he commanded had been constantly employed on a particular description of service, having no reference to such inquiries, but which occupied nearly all his time.

It was only, therefore, at casual intervals that he could make use of the means placed in his hands, and many interesting opportunities were thus lost merely for want of adequate leisure to profit by them.

The only stations, accordingly, at which the pendulum was swung in a satisfactory manner, were, 1st, the Galapagos Islands in the Pacific, lying 32½ miles N. of the Equator; 2ndly, St. Blas, on the N.W. shore of Mexico, in lat. 21½° N., and not far from California; and, lastly, Rio de Janeiro, the capital of Brazil, in lat. 22°·55′ S.

The ellipticity of the earth, resulting from the experiments made at the Gallipagos, is [illegible]; but it must be observed that, as the nature of the ground on which the pendulum was here swung, was totally different from that of the stations at which it was swung in England, this result is not to be depended upon as giving a true expression for the ellipticity; since whenever a comparison is to be made between different stations on which the same pendulum has been swung, the circumstances ought always to be as nearly alike as possible in all that relates to the density of the ground. At the Galapagos, however, every thing was volcanic, and the islands were surrounded by a deep sea, whereas at the English stations the rocks were mostly primitive, and were surrounded by land. At the second station, San Blas, there were two series of experiments made, the first by Capt. Hall, which gave an ellipticity of [illegible], and the second by Mr. Henry Foster, one of his officers, [illegible]. These two series, however, were performed under circumstances so different, as to account sufficiently for the want of exact agreement in the results. During the first the temperature was steady, the sky always clear, and the rate of the clock uniform; but before the second series was well commenced the rainy season set in with great violence, the sky became overcast, few stars could be seen, the temperature fluctuated, and the rate of the clock became unsteady. The relative degree of credit, therefore, which attaches to the two series, is as 397 at 47, the amount of the respective factors, or nearly as 8 to 1, and we may assume the correct ellipticity at [illegible]. At Rio de Janeiro, by Captain Hall's series, the ellipticity comes out [illegible], and by Mr. Foster's [illegible]. The sum of the factors in this case being 148 and 304, or nearly as 261 in favour of Mr. Foster's experiments, the correct ellipticity may be taken at [illegible].

Captain Hall is of opinion that it would contribute materially to the determination of this question if the same pendulum were to be swung at places resembling the Galapagos, in geological character, such as the Canaries and Azores, Madeira, St. Helena, the Isle of France, and various islands in the Eastern Ocean; besides swinging it at stations resembling those of Captain Kater in this country, especially at the Cape of Good Hope, and at a port in the Falkland Islands, which happen to lie in the correspondent latitude to that of London, and afterwards as a check on the adjacent main land of Patagonia.

Captain Hall announced his intention of instituting, in the mean

time, some experiments for determining the actual effect on the vibrations of the pendulum, by changes of temperature, independently of any theoretical allowance.

The portion of Captain Hall's letter, which was not adapted to public reading, consists of six series of observations at London, the Galapagos, San Blas, and Rio de Janeiro. These are given in the fullest detail, together with every attendant circumstance likely to have any effect on the experiments.

Second Part of the Paper on the Nerves of the Orbit. By Charles Bell, *Esq. Communicated by Sir* Humphry Davy, *Bart. P.R.S.* Read June 19, 1823. [*Phil. Trans.* 1823, *p.* 289.]

This is a paper in continuation.—In this part the author states that there are six nerves of the brain which go to the eye, while there are in all only nine nerves coming from the brain. He proceeds to show that each nerve has a distinct or appropriate function.

The branch of the fifth nerve, called Ophthalmicus, bestows upon the parts within the orbit and the coats and surfaces of the eye, sensibility to pain, and that modification of sensibility which causes the winking and rapid motions of the eyelids and eyeball, and produces tears. It is shown that by the loss of this nerve the eye is deprived of its sensibility to touch and irritation, while it continues to enjoy the sense of vision, and the motions of the cye and eyelids remain perfect.

Having shown that the motions of the eyelids depend on a branch of the seventh nerve, and not on the fifth, as hitherto supposed, and having proved in the former part of the paper that there is a corresponding motion of the eyeball and eyelids, necessary for the preservation of the organ, he proceeds to show that this connexion is established through the fourth nerve; that the fourth, or trochlearis, goes to that muscle which performs the instinctive and insensible motions of the eyeball, in sympathy with those of the muscles of respiration, and consequently with the eyelids.

Having thus shown that the fourth, and a branch of the seventh, as enumerated by authors, perform the instinctive and involuntary motions of the eyeball and eyelids, he then explains the office of the third and sixth to be exclusively for the voluntary motions of the eye, and for directing the eyeball in subservience to its office of vision.

By the discovery of the peculiar functions of the fourth, fifth, and seventh, the nerves of the head become thus arranged according to their uses:—

The first is the olfactory nerve.

The second is the nerve of vision.

The third is for the voluntary motions of the eye.

The fourth for the instinctive motions of the eye.

The fifth entirely for sensation of the head generally, and for the regulation of certain motions of the jaws.

The sixth for the motions of one muscle of the eye.

The seventh for the voluntary and involuntary motions of the muscles of the face.

The eighth is the great respiratory nerve.

The ninth, for the voluntary motions of the tongue.

The nerve sometimes enumerated as the tenth of the brain, is properly the first spinal nerve: like those of the spine it has a distinct double root, and is a nerve both of sensation and of motion.

In the end of the paper certain conclusions are drawn, which have reference to the subjects of the preceding papers of the same author. And here the author advocates the importance of anatomical investigation as superior to experiments on living animals, affirming that erroneous opinions in physiology have been sometimes strengthened instead of corrected by such experiments.

An Account of Experiments made with an Invariable Pendulum at New South Wales, by Major-General Sir Thomas Brisbane, *K.C.B. F.R.S. Communicated by Captain* Henry Kater, *F.R.S., in a Letter to Sir* Humphry Davy, *Bart. P.R.S.* Read June 19, 1823. [*Phil. Trans.* 1823, *p.* 308.]

In this communication, laid before the Royal Society at the request of Sir Thomas Brisbane, Captain Kater gives the results of experiments made by Sir Thomas Brisbane and Mr. Dunlop, at Paramatta, in New South Wales; and by Sir Thomas Brisbane, Captain Kater, and Mr. Rumker, in Portland Place, London, with an invariable pendulum belonging to the Board of Longitude.

The number of vibrations made by this pendulum in a mean solar day at London, in latitude 51° 31′ 8″ 4, at the temperature of 60°, and reduced to a vacuum, was found to be 86090·17: at Paramatta, in latitude 33° 48′ 43″ S., by Sir Thomas Brisbane's experiments 86021·59; and by those of Mr. Dunlop 86022·21.

Captain Kater, after detailing these observations, proceeds to deduce the compression, and finds it, by comparing the vibrations at London with those resulting from Sir Thomas Brisbane's experiments at Paramatta, to be [illegible]; by Mr. Dunlop's [illegible].

By the comparison of the vibrations at Unst with those at Paramatta, Sir Thomas Brisbane's experiments give [illegible], and those of Mr. Dunlop [illegible] for the compression.

But the author remarks that these numbers are not to be deemed conclusive, as a small alteration in the number of vibrations made by the pendulum would occasion a considerable difference in the fraction indicating the compression.

Observations and Experiments on the Daily Variation of the Horizontal and Dipping Needles under a reduced Directive Power. By Peter Barlow, *Esq. F.R.S. of the Royal Military Academy. Communicated by* Davies Gilbert, *Esq. V.P.R.S.* Read June 12, 1823. [*Phil. Trans.* 1823, *p.* 326.]

By disposing magnets so as partly to counteract the influence of the earth's polarity on a magnetic needle, the author suspected that its daily variation might possibly exhibit itself in a very increased degree; and in experimentally prosecuting the idea, he found it to be the case to a very considerable extent in regard to the horizontal needle; and to take place also, though less satisfactorily, with the dipping-needle.

In the former experiment a finely suspended horizontal needle was used, the directive power of which was reduced by two magnets, properly placed for the purpose; a deviation of 3° 15′ was thus obtained at 11 A.M., which decreased to a late hour in the evening. The needle was kept in the same position for three days, with some change of directive power, with similar general results. After adverting to a difference in the direction of the variation in and out of doors, Mr. Barlow details the results of several experiments, which lead him to the following conclusions:—that while the north end of the needle is directed to any point from the S. to N.N.W., its motion during the forenoon advances towards the N.; and while directed towards any point between the N. and S.S.E. its motion is still to some point between the N. and N.N.W., so that there ought to be some direction between those limits in which the daily motion is a minimum: but whether this is a fixed direction during the year, or whether it has any vibratory motion as the sun changes its declination, is a question requiring further experiments to determine. Another conclusion which the author draws is, that the daily change is not produced by a general deflection of the directive power of the earth, but by an increase and decrease of attraction of some point between the N. and N.N.E., or between the S. and S.S.E.

The variation of the adjusted dipping-needle did not indicate the gradually increasing and decreasing power manifested by the horizontal needle; and although it evidently is subject to diurnal change, the law of that change remains to be determined.

On the Diurnal Deviations of the Horizontal Needle when under the influence of Magnets. By Samuel Hunter Christie, *Esq. M.A. Fellow of the Cambridge Philosophical Society: of the Royal Military Academy. Communicated by Sir* Humphry Davy, *Bart. P.R.S.* Read June 19, 1823. [*Phil. Trans.* 1823, *p.* 342.]

This paper contains a detailed account of a series of observations on the diurnal magnetic variation made upon the principle pointed out by Mr. Barlow, in his communication already before the Society. In regard, however, to the arrangement adopted by the author, it

differed from that of Mr. Barlow, for instead of applying a magnet in the horizontal plane of the needle he placed two in the line of the dip, having their poles in the reverse position to those of the needle. In describing the results of his experiment, Mr. Christie calls the south pole of a magnet that which points to the north pole of the earth, so that the north end is the south pole, and the south end the north pole of his magnetic needles. In stating the deviations from any point considered as zero, those which he places in the direction of the sun's apparent daily motion are considered minus, and those in a contrary direction plus, whatever may be the position of the needle. The results of Mr. Christie's observations are given in the form of tables, the first of which extend from March 26 to March 31, and exhibit manifest deviation towards the east before eight in the morning, and the greatest westerly deviation about one P.M. In a second series of observations, with another needle, the directive force of which was to the power as 1·63 to 1, the times of the greatest easterly and westerly deviation agreed with the former, but the easterly was greater compared with the westerly. In a third series the suspension of the needle was improved, and the magnets were so adjusted as to render its directive force as 0·68 to 1. With this needle the observations were made nearly every hour, from the 5th to the 12th of April, and the mean results give the greatest easterly variation at $7^h\ 45^m$, the time of no deviation at $9^h\ 26^m$; the greatest westerly deviation took place at $1^h\ 24^m$, the time of zero being $6^h\ 40^m$, and the total daily variation amounting to $3^\circ\ 41'$. After adverting to the influence of the weather, to irregularities referrible to the electric state of clouds, and to other causes of anomalies in prosecuting his inquiries, the author proceeds to determine the results of a numerous series of observations made as the former, but in which the position of the needle was somewhat altered, as well as that of the bar magnets; and to examine into the daily changes which take place at the points at which a needle is retained in equilibrio by two bar magnets, which he is led to believe arise from an actual change of intensity in the terrestrial forces, as well as from a change in their directions.

The author concludes his paper with remarks upon the influence of temperature in diminishing the power of the magnets, and infers that solar heat exceeds any other in producing such an effect; but that changes of temperature are not the only cause of the variations which take place in the points of equilibrium, is shown by their occurring independent of the temperature of the magnets.

On Fossil Shells. By Lewis Weston Dillwyn, *Esq. F.R.S. In a Letter addressed to Sir* Humphry Davy, *Bart. P.R.S.* Read June 5, 1823. [*Phil. Trans.* 1823, *p.* 393.]

In describing the shell fish supposed to yield the Tyrian dye, Pliny has adverted to its power of boring the shells of other fish; and Lamarck says that all mollusca, whose shells have a notch at the base of their

apertures, are possessed of similar powers. In the other genera of turbinated univalves, the aperture, instead of being notched, is entire, and they have all been proved to be herbivorous. Every turbinated univalve which Mr. Dillwyn has examined of the older beds, from the transition limestone to the lias, belongs to these herbivorous genera, and the family still inhabits our land and waters. On the contrary, all the carnivorous genera abound in the strata above the chalk, but are very rare in the secondary strata. In recent shells small holes bored by the predaceous Trachelipoda are common; and Mr. Dillwyn has observed similar holes in fossils from the London clay, but never in those of the older formations; and he thinks that the whole family of carnivorous Trachelipoda are very rare in all those strata where the Ammonites and other Nautilidæ abound. Ammonites, and the other principal multilocular genera, appear to have become extinct in northern latitudes when the chalk formation was completed: but a few of the Nautilidæ still inhabit the Southern Ocean. Mr. Dillwyn further observes, that all the marine genera of the herbivorous Trachelipoda, to which the fossil species belong, have an operculum, and that the carnivorous species of the secondary strata agree with them in this particular, though the unoperculated genera abound in the London clay. Although fossil Nautilidæ are common in the secondary strata of the United States, they are said not to have been found in South America. Hence, says the author, it may be queried whether the Cephalopoda were not confined to the more northern latitudes when the chalk formation was completed; and whether a decrease in the earth's temperature at that period may not have occasioned the entire destruction of some genera, and the migration of others to the south.

On the apparent Magnetism of Metallic Titanium. By William Hyde Wollaston, *M.D. V.P.R.S.* Read June 19, 1823. [*Phil. Trans.* 1823, *p.* 400.]

Adverting to his statement respecting the action of the magnet upon metallic titanium, published in the first part of the Philosophical Transactions for this year, which refers it to adhering iron, Dr. Wollaston observes, that in subsequent examinations he has found the crystals of that metal slightly attractable, although he had formerly considered them as not thus influenced when apparently perfectly pure. From some comparative trials, however, he finds that the magnetic power thus exhibited would be conferred by the presence of about $\frac{1}{150}$th part of iron alloyed with the titanium; and there is every reason to suspect that the latter metal might be thus contaminated. This is rendered additionally probable by the action of tests upon the solutions of the supposed pure titanium; and upon the whole, Dr. Wollaston thinks that we should not be warranted in classing titanium with the magnetic metals.

An Account of the Effect of Mercurial Vapours on the Crew of His Majesty's Ship Triumph, in the Year 1810. *By* William Burnett, *M.D. one of the Medical Commissioners of the Navy, formerly Physician and Inspector of Hospitals to the Mediterranean Fleet. Communicated by* Matthew Baillie, *M.D. F.R.S.* Read June 19, 1823. [*Phil. Trans.* 1823, *p.* 402.]

The Triumph arrived at Cadiz in 1810, and in the following March a Spanish vessel, laden with quicksilver, was wrecked under the batteries, then in possession of the French. The Triumph's boats were sent to her assistance, and about 130 tons of the quicksilver carried on board. The metal was secured in bladders packed in barrels, but the bladders having been wetted grew rotten, and the metal escaped in large quantities, got mixed with the provisions, and very soon affected the crew with ptyalism, ulcerated throats, &c. The different animals on board were also affected. From the extent of the mischief it was evident that the air of the confined part of the vessel contained mercurial vapour, and accordingly those who slept and messed in the orlop and lower decks were more severely affected than those chiefly confined to the upper deck; while the men who lived and slept chiefly under the forecastle, escaped with a slight affection of the gums.

On the Astronomical Refractions. By J. Ivory, *A.M. F.R.S.* Read June 19, 1823. [*Phil. Trans.* 1823, *p.* 409.]

The ancients, Mr. Ivory observes, were acquainted with the existence of atmospherical refraction; but the first that ascertained its magnitude with tolerable accuracy, and employed it in his calculations, was Tycho Brahé. Cassini attempted to compute the refraction upon optical principles, and upon the hypothesis of an uniform medium of uniform density,—a supposition which, though very simple, is sufficiently correct to a considerable extent. The next step was to imagine an atmosphere of a density uniformly decreasing as the height increases. Kramp was still more accurate in attending to the true effects of pressure and change of temperature; his methods have been improved and extended with great sagacity by Laplace, and the tables founded on his computations are perhaps the best in existence with respect to the value of the mean refractions.

An uniform atmosphere must be supposed to be five miles in height; an atmosphere uniformly decreasing in density ten. Kramp and Laplace consider it as infinite. The former limits would make the horizontal refraction less than the truth; the latter supposition much greater. Mr. Ivory is inclined to suppose some considerably extended, though finite height, which shall give the true refraction at the horizon, and which will probably be also correct for all other cases; and he thinks it not superfluous to inquire, whether such an atmosphere would afford results sensibly different from those of an atmosphere of infinite extent. The phenomena of twilight and of

meteors indicate, he observes, a height of forty or fifty miles, at which the atmosphere is dense enough to reflect a sensible quantity of light. At the height of about 25,000 miles the centrifugal force would become equal to the gravitation of the air, and the equilibrium would be no longer possible.

But the great reason that prevents our supposing an atmosphere infinitely extended, is the coldness of the upper regions. Mr. Ivory considers the elastic force as disappearing from the effect of cold upon the temperature, is 266 centigrade degrees below the freezing point; and he observes, that if the decrease uniformly amounted to a degree in 95 (English) fathoms, as it appeared to do in Gay Lussac's aerostatic observation, the whole height ought not to exceed 29 miles; consequently he thinks that the thermometer must fall at a slower rate in the higher than in the lower parts of the atmosphere.

Mr. Ivory proceeds to investigate the motion of light according to the laws of central forces, and to the experiments of Hauksbee and others on the refractive density of the air. He first shows that the formula employed by the French astronomers, as far as 74° from the zenith, is deducible from any imaginable law respecting the constitution of the atmosphere; and he gives, for example, the mode of obtaining it from that of Cassini.

He next considers the case of an equable variation of temperature in ascending, which he thinks is rendered probable, as the law of nature, by observations of Gay Lussac and others; and he inquires into the general methods of integrating the expressions of the refraction in such cases, according to the methods already employed by Kramp and Laplace. He examines, on various suppositions respecting the height of the atmosphere as connected with various laws expressing the progressive temperatures, beginning always from that which is observable at the earth's surface the amount of the horizontal refraction, which he finds not so materially affected by these suppositions as to enable us to decide with certainty, from observation, which of them ought to be preferred.

Upon these foundations the author investigates the actual magnitude of the refraction under different circumstances, by means of several infinite series, of which he computes the values; and he compares his results with those which have been observed by astronomers. The ingenious hypothesis of Laplace, he remarks, gives us a height of 59½ fathoms for a depression of a centigrade degree, or 197 feet for 1° of Fahrenheit, which, he says, is little more than two thirds of the height actually corresponding to this depression; and the French table, he says, was computed for the foregoing point, and then altered proportionally throughout its extent for a difference of 10 centigrade degrees. As far as 86° from the zenith, Mr. Ivory's computations agree very accurately with those of Bessel; and further, this celebrated astronomer does not recommend the adoption of his table. The comparison of the new table with the observations of the French Astronomers, and of Dr. Brinkley, appears to be highly favourable to the accuracy of Mr. Ivory's results.

Observations on Air found in the Pleura, in a Case of Pneumato-thorax; with Experiments on the Absorption of different kinds of Air introduced into the Pleura. By John Davy, *M.D. F.R.S.* Read June 6, 1823. [*Phil. Trans.* 1823, *p.* 496.]

In this paper Dr. Davy gives a detailed account of the symptoms produced by the above disease, and of the appearance after death, in a man of thirty years of age. He also adverts to the composition of the air found in the lungs, which was collected by immersing the body in water, and puncturing the pleura, when it issued in the enormous quantity of 225 cubic inches. It was without smell, and extingui hed flame, and was not inflammable. It consisted of 8 carbonic acid, and 92 azotic gas per cent.; and the author considers it as derived from the atmosphere by a morbid communication, which was discovered on dissection, between the pleura and atmosphere through the medium of the lung. To determine the mode in which its change of composition had been effected, Dr. Davy inflated the right pleura of a dog with atmospheric air, and killed the animal after 48 hours. On examining the air, the oxygen was found absorbed in larger proportion than the azote, which accounts for the accumulation of the latter gas in the preceding instance. To ascertain how far carbonic acid is absorbed by the pleura, a mixture of 80 parts of common air, and 20 of carbonic acid, was injected into the right pleura of a dog. After two days the animal appeared well, and a mixture of 75 of air and 25 of carbonic acid was thrown into the left pleura. Twenty-four hours after, the dog was killed, and the result was, that during a sojourn of three days in the pleura the oxygen had been absorbed in greater proportion than the carbonic acid, and the latter in a greater degree than the azote. The power thus exhibited by the pleura of absorbing one kind of gas more than another, without reference to their solubility in water, induced the author to institute some similar experiments with hydrogen, nitrous oxide, and nitrous gas. A mixture of carbonic acid and hydrogen thus applied did not appear to affect the health of the animal. A mixture of azote and nitrous gas killed the animal in five hours. In the former case the hydrogen disappeared, and its place was supplied by a small quantity of azote. As the author's experiments induce him to believe that no air exists in the pleura in a healthy state, he is led to suppose that its appearance in this case is referrible to secretion.—In a note annexed to this paper, Dr. Davy expresses his doubt as to the existence of any free carbonic acid in the blood; since he could, in two experiments made for the purpose, obtain none by means of the air-pump.

On Bitumen in Stones. By the Right Hon. George Knox, *F.R.S.* Read June 12, 1823. [*Phil. Trans.* 1823, *p.* 517.]

In this paper Mr. Knox details a series of experiments upon a great variety of mineral products, tending to show in them the frequent existence of bitumen, or some analogous substance, and he

calls the attention of analysts to this subject, as having frequently misled them in respect to the loss obtained by subjecting minerals to heat, which they have too commonly ascribed to the mere evaporation of aqueous matter.

Mr. Knox found bitumen in nearly all the minerals belonging to Werner's Flötz Trap formation. He also found it in a few of the substances appertaining to the older rocks; but in the latter case it was smaller in quantity, and less easily separable.

In conclusion, the author recommends distillation as a process to be resorted to in all analyses of stony substances, with a view of obtaining the liquid bitumen which they may have contained, and also of estimating the proportion of carbon evolved in the state of gas. The residuum, he observes, should be carefully examined for the remaining carbon.

On certain Changes which appear to have taken place in the Positions of some of the principal fixed Stars. By John Pond, *Astronomer Royal, F.R.S.* Read June 19, 1823. [*Phil. Trans.* 1823, *p.* 529.]

The Croonian Lecture. On the Internal Structure of the Human Brain, when examined in the Microscope, as compared with that of Fishes, Insects and Worms. By Sir Everard Home, *Bart. V.P.R.S.* Read November 20, 1823. [*Phil. Trans.* 1824, *p.* 1.]

In this lecture the author pursues his researches respecting the anatomy of the human brain, and compares it with that of fishes, insects, and worms, in the hope of developing the connexion between the action of the nerves and the motion of the muscles. To obviate the sources of error attendant upon the usual methods of dissecting and examining the brain, a portion of it in a very recent state was submitted to the microscope, after having been immersed in distilled water. Rows of globules were thus detected passing in straight lines from the circumference of the cortical substance into the medullary portion, the appearance of which is shown by Mr. Bauer in an annexed drawing. From a representation, also by Mr. Bauer, of the tench, it appears that its relative proportion to the size of the animal is smaller than in the bird,—that it has a central cavity and a nodulated basis. In insects the brain contains, and its principal portion is connected by, nervous chords, with what is usually called a ganglion, but which, when examined accurately, is found to resemble the brain in texture, and which, from the office of the nerves it sends off, the author considers, as *Medulla oblongata*. Below this is a regular line of ganglions united by a double nerve, the details of these structures being illustrated by annexed drawings.

Among insects the bee has the largest proportion of brain relative to the size of its body. In the moth and caterpillar it is smaller, but similar in structure, as also in the lobster. In the garden snail the brain is relatively larger than in the bee, but there are no ganglions, although the structure of the *Medulla spinalis* is the same.

In all animals the minute structure of the brain, as detected by the microscope, appears in a measure similar, consisting of globules, and a more or less fluid and often elastic transparent matter. The ganglions appear also similarly composed in all the animal tribes of a congeries of nervous fibres.

Some Observations on the Migration of Birds. By the late Edward Jenner, *M.D. F.R.S.*; *with an Introductory Letter to Sir* Humphry Davy, *Bart. P.R.S. By the Rev.* G. C. Jenner. Read November 27, 1823. [*Phil. Trans.* 1824, *p.* 11.]

The author's intention in this paper is not to give a general history of the migration of birds, but to adduce some hitherto unnoticed facts respecting the causes which excite the bird at certain seasons of the year to quit one country for another. Among these the most prominent are certain changes in the generative organs, and the necessity of a climate or country where they can be better accommodated with succours for their infant brood than in that from which they depart. He also adduces facts to prove that their departure from this country is not in consequence of change of temperature or scarcity of food, but the result of the accomplishment of their errand, namely, the incubation and rearing their young.

The author then offers some remarks on the winter birds of passage, tending to show that they quit this country in spring in quest of a situation better adapted to their intended purpose, and that they are actuated by the same impulse in leaving this country that causes the spring birds to come to it, and not by want of food. That the emigration of the winter birds is less complete than that of the spring migrators, and that some species, especially the wild duck and wood-pigeon, breed here.

Redwings and fieldfares appear to be the most regular and uniform in their appearance and disappearance, and never seem to risk the trial of incubation here.

Dr. Jenner remarks, that in severe and long-continued frosts, birds often quit the country through want of food, but that they return upon the approach of more temperate weather, which is announced by their reappearance. The arrival of water birds, on the contrary, forebodes the approach of intense frost.

The author then offers some observations on the singing of birds, and details some additional facts and particulars respecting the different sizes of the generative organs of birds, as they appear at different seasons of the year.

On the Nature of the Acid and Saline Matters usually existing in the Stomachs of Animals. By William Prout, *M.D. F.R.S.* Read December 11, 1823. [*Phil. Trans.* 1824, *p.* 45.]

The object of this communication is to prove that the free acid usually existing in the stomachs of animals is the muriatic acid, and that the salts present are alkaline muriates.

The contents of a rabbit's stomach were digested in distilled water, and the clear portion of liquid thus obtained divided into four parts. The first was evaporated to dryness, and the quantity of muriatic acid present in the residuary fixed salts determined by nitrate of silver; the second was supersaturated with potash, evaporated and decomposed by nitrate of silver as before, by which the total quantity of muriatic acid in the fluid was ascertained; the third was neutralized by a solution of potash of known strength, and the required quantity accurately noticed. This gave the proportion of free acid present; and by adding this to that in union with a fixed alkali, as above determined, and subtracting the sum from the total quantity of muriatic acid present, the proportion of acid in union with ammonia was estimated. But as a check to this result, the third neutralized portion was evaporated to dryness, and the muriate of ammonia expelled by heat; the quantity of muriatic acid left in union with the fixed alkali was then determined as before; and by subtracting this from the total quantity, the quantity in union with ammonia was determined.

From such experiments the author concludes, that no inconsiderable quantity of unsaturated muriatic acid exists in the stomachs of animals during digestion; and from the examination of the fluid ejected from the human stomach in a case of dyspepsia, he infers that there also the muriatic acid performs the same office.

On the North Polar Distances of the principal fixed Stars. By John Brinkley, *D.D. F.R.S. &c. Andrews Professor of Astronomy in the University of Dublin.* Read December 18, 1823. [*Phil. Trans.* 1824, *p.* 50.]

The author observes, that of the recent catalogues that have been formed of the principal fixed stars, two, those of Dublin and Greenwich, agree very exactly. That of M. Bessel differs considerably; but the differences are such that they would agree by a modification of the co-efficients of refraction employed for correcting the observations. Mr. Pond, he says, has applied the refractions of Bradley to the instrument of Dublin; he himself thinks it more safe to determine the refraction for each place from its own instruments; and he objects to the reasoning by which Mr. Pond has endeavoured to prove the existence of a flexure in the tube of the instrument of Dublin.

Dr. Brinkley makes the mean difference of the stars of Greenwich and Dublin for 1813 only a few tenths of a second; for 1823, still less. Neither the Dublin Catalogue, nor any other more extensive comparison, affords, in his opinion, a confirmation of the hypothesis of a general southern motion, which he is therefore inclined to attribute principally to a slight inaccuracy of the Greenwich Catalogue for 1813.

The comparisons unfavourable to the southern motion are those of Bradley's observations at Wanstead, in 1728, and some French ob-

servations, in 1740; of Maskelyne's at Schehalien, in 1774; General Mudge's, in 1802; and General Lambton's, in 1805; as well as of Piazzi's Catalogue. Mr. Pond's observations at Westbury agree too little with the Catalogue of Greenwich, he thinks, to be of any use in the inquiry; and Mechain's are opposed to others made by better instruments.

Dr. Brinkley does not think it an argument in favour of Bessel's refractions, that they give the obliquity of the ecliptic the same for both solstices; he rather supposes some particular equation is required for the solar refraction, Bessel's refraction at low altitudes being manifestly too large for the stars.

The question of parallax Dr. Brinkley still reserves for future discussion.

On the Figure requisite to maintain the Equilibrium of a Homogeneous Fluid Mass that revolves upon an Axis. By James Ivory, *A.M. F.R.S.* Read December 18, 1823. [*Phil. Trans.* 1824, *p.* 85.]

The author enumerates the various steps by which Sir Isaac Newton, M^{c}Laurin, and Laplace have carried the theory of the equilibrium of a revolving fluid very near to perfection, but he observes that they have generally supposed the spheroid to differ but little from a sphere; and he proceeds in the present paper to investigate the figure "by a direct analysis, in which no arbitrary supposition is admitted."

Mr. Ivory thinks it necessary to distinguish carefully two separate cases; the first is when the particles of the fluid do not attract one another, and the second when the particles are endued with attractive powers. These, he says, are plainly two cases that are essentially different from one another; for in the first, a stratum added induces no other change than an increase of pressure caused by the action of the accelerating forces at the surface; but in the second, besides the pressure, a new force is introduced, arising from the mutual attraction between the matter of the stratum and the fluid mass to which it is added.

Mr. Ivory gives two different methods of investigating the fundamental laws of this equilibrium, the one which is the newest and most simple being contained in two propositions.

First, If a homogeneous fluid body revolving about an axis be in equilibrio by the attraction of its particles, any other mass of the same fluid having a similar figure, and revolving in the same time about an axis similarly placed, will likewise be in equilibrio by the attraction of its particles.

The proof is easily deduced from the well known properties of an attraction inversely proportioned to the square of the distance.

Secondly, If a homogeneous fluid mass revolve about an axis, and be in equilibrio by the attraction of its particles, all the level surfaces will be similar to the outer surface; and any stratum of the fluid contained between two level surfaces will attract particles in the in-

side (or within the interior surface of the stratum) with equal force in opposite directions.

On the Corrosion of Copper-sheeting by Sea-water, and on Methods of preventing this Effect; and on their Application to Ships of War and other Ships. By Sir Humphry Davy, *Bart. P.R.S.* Read January 22, 1824. [*Phil. Trans.* 1824, *p.* 151.]

When copper sheeting, however pure the metal may be, is exposed to sea-water, a green rust is formed upon it; which, when washed off, is replaced by a similar substance, till the whole of the metal is thus destroyed by corrosion. To prevent this effect, the President avails himself of the modification of chemical affinities, derived from electrical power; and in pursuing his researches in relation to this subject, he found the above-mentioned action upon copper counteracted by any weak negative electricity easily excited in it by the contact of a surface of tin not exceeding $\frac{1}{100}$th that of the copper, and made part of an electric circuit in sea-water. Other metals may be substituted, but the facility with which a perfect contact is made by solder with tin, and the facility with which its submuriate detaches from the metal, induce Sir Humphry Davy to regard it as best adapted to the purpose. He observes, further, that the cause which prevents the oxidation of the copper will also probably prevent the adhesion of marine animals and of vegetables. After adverting to the unequivocal and satisfactory results of his experiment made upon a small scale, the author states that the Lords Commissioners of the Admiralty have enabled him to make arrangements for pursuing them upon a very extended plan.

A finite and exact Expression for the Refraction of an Atmosphere nearly resembling that of the Earth. By Thomas Young, *M.D. For. Sec. R.S.* Read February 5, 1824. [*Phil. Trans.* 1824, *p.* 159.]

Having shown that if the pressure of the atmosphere be represented either by the square, or by the cube of the square root of the density, the astronomical refraction may be attained in a finite equation; and having adverted to Mr. Ivory's computation of the refraction with the assistance of converging series, and several transformations from an equation which expresses the pressure in terms of the density and of its square, Dr. Young proceeds to observe, that if we substitute for the simple density the cube of its square root, we shall represent the constitution of the most important part of the atmosphere with equal accuracy, although this expression supposes the total height somewhat smaller than the truth; and that we shall thus obtain a direct equation for the refraction, which agrees very nearly with Mr. Ivory's table, and still more accurately with that in the Nautical Almanac, and with the French tables.

At the horizon the refraction is equal to $33' \, 49''{\cdot}5$, which is only $1''{\cdot}5$ less than the quantity assigned by the French tables and in the

Nautical Almanac; while Mr. Ivory makes it 34′ 17″·5. Again, for the altitude 5° 44′ 21″, we obtain 8′ 49″·5 for the refraction; while the Nautical Almanac gives us 8′ 53″, and Mr. Ivory's table 8′ 49″·6. The author, however, observes that there is no reason for proceeding to compute a new table by this formula, since the method employed for that in the Nautical Almanac is in all common cases more compendious; and even if it were desired to represent Mr. Ivory's table by the approximation there employed, we might obtain the same results, with an error scarcely exceeding a single second, from an equation of the same form.

The Bakerian Lecture. On certain Motions produced in Fluid Conductors when transmitting the Electric Current. By J. F. W. Herschel, *Esq. F.R.S.* Read February 12, 1824. [*Phil. Trans* 1824, *p.* 162.]

In the first paragraphs of this lecture, Mr. Herschel describes the phenomena that result on placing a portion of mercury covered with sulphuric acid between the voltaic poles immersed on opposite sides of the globule of metal, but in contact with the acid only. They consist in active motion of those particles of the acid in contact with the mercury, while the superficial molecules of the metal continually radiate from the point nearest the negative pole, and darting to the positive pole return along the axis. The author particularly notices several singular appearances resulting from this current, and shows them to be independent of any electro-magnetic vortices, to which, at first sight, they present considerable analogy. They are incomparably more forcible, in proportion to the electric powers used, than the motions produced by the action of magnets; hence, they furnish an extremely sensible test of the development of feeble Voltaic powers not easily rendered sensible by other means.

The author next describes the appearances observed in cases where other liquids and metals are used, and adverts to the influence of several causes upon the uniformity of the results. Among these, impurity in the mercury is especially noticed, which should not only be carefully distilled, but also well washed with dilute nitric acid. Mercury thus prepared, and placed in the current as before, exhibits phenomena varying with the nature of the liquid;—generally speaking, currents are produced radiating from the point nearest the negative pole, which are most violent in acids, and less in saline solutions, in proportion as the electro-positive energy of the base is greater. In many liquids a counter-current from the positive pole is observed; but if either pole be brought in contact with the mercury, no currents are observed from the point of contact, but strong ones are perceived to radiate from the other. If the negative pole touch it, it amalgamates with the mercury, which remains bright; if the positive, the mercury rapidly oxidizes: and in both cases currents are produced.

Mr. Herschel proceeds to observe, that when mercury is electrized

in saline solutions, its properties are generally altered; and he describes at length the phenomena thus presented in a solution of sulphate of soda, which were peculiar and apparently perplexing, but which he found to depend upon the presence of amalgam of sodium counteracting the effect of the negative pole, and exalting that of the positive in proportion to its quantity, until it overcomes and even reverses it. That sodium is actually present in these cases the author shows by the following experiment:—Having detached the negative wire, he touched the mercury, now lying quiet in the liquid, with a platinum or copper wire, and a violent action instantly began. The mercury rushed to the wire in a superficial current, and it gave off abundance of hydrogen; the sodium, wire, and liquid, forming a voltaic combination sufficiently powerful to decompose the water.

The author next proceeds to investigate more minutely the effects of different metals in their contact and amalgamation with mercury, employing solutions of the caustic alkalies for the conducting liquids, which have the advantage of producing no currents in pure mercury so long as neither pole is in contact with it. In liquid potash a contact with the negative pole, of a single second's continuance, imparted to 100 grains of mercury the property of rotating violently from the positive to the negative pole, when the circuit was completed in the liquid alone. The rotation was even sensible when the quantity of potassium did not probably exceed a millionth part of the whole mass. With sodium similar effects were observed; and even where the proportion of sodium to mercury was only as 1 to 1,600,000, a feeble motion was sensible.

The influence of barium, strontium, calcium and magnesium, and of zinc, lead, tin and iron, is next described, the alloys of these metals being all possessed of the positive property. Copper, on the other hand, does not communicate motion, though present in considerable proportion; nor do bismuth, silver, nor gold.

Mr. Herschel concludes this lecture with some general and theoretical observations and deductions, founded on his experimental inquiries. These relate principally to the exceedingly minute proportions of extraneous matter capable of communicating sensible mechanical motions, and properties of a definite character, to the body they are mixed with. When we see energies so intense exerted by the ordinary forms of matter, we may, says the author, reasonably ask what evidence we have for the imponderability of any of the powerful agents to which so large a part of the activity of material bodies seems to be owing.

Among the essential conditions of the phenomena, the author particularly adverts to the vast difference of conducting power between the metallic bodies set in motion, and the liquid under which they are immersed; to the necessity of the perfect immiscibility of the conducting fluids, so as to render the transition of one to the other quite sudden; and to a certain chemical or electrical relation between

them. Under these conditions, Mr. Herschel observes, the phenomenon may admit of explanation, from what we already know of the passage of electricity through conductors, and the high attractive and repulsive powers of the two electricities *inter se*. A body so highly positive as potassium, present in the mercury, may, for instance, have its natural electrical state exalted by its vicinity to the positive pole; and being thus repelled, may take the only course the resistance of the metal on the one hand, and attraction of cohesion on the other, will permit, viz. along the surface, to recede from the positive pole; it may even act as a carrier to the positive electricity, which may adhere to it too strongly to be transmitted through the mercury, and when arrived at the opposite side of the globule may there, by the influence of the opposite pole, lose its exalted electrical state. Such an explanation, however, is not without its difficulties; and although another is open to us, that of considering the action which takes place at the common surface of two unequally conducting media as dependent upon a new power of the electric current, bearing some analogy to magnetic action, yet this, in the present state of the investigation, must be regarded not only as a bold, but vague hypothesis.

Experiments and Observations on the Development of Magnetical Properties in Steel and Iron by Percussion:—Part II. By William Scoresby, *Jun. F.R.S.E. &c. Communicated by Sir* Humphry Davy, *Bart. Pres. R.S.* Read January 29, 1824. [*Phil. Trans.* 1824, *p.* 197.]

After adverting to the general results of his former inquiries, the author observes that his principal objects on the present occasion were to endeavour, by auxiliary rods of iron, to increase the degree of magnetism; and to ascertain on what circumstances, as to the magnitude of the iron rods, and the quality, size, and temper of the steel wires, the utmost success of the method depends.

He formerly used a single iron rod, upon which the steel bars were hammered, both being in a vertical position. He now places the steel wire between two rods of iron, and subjecting it, through the medium of the upper rod, to percussion, derives the advantage of the magnetism of both rods of iron acting at the same time upon both its poles. The rods he used were of the respective lengths of three and one foot, and an inch diameter; and the upper end of the larger rod and the lower one of the smaller rod were made conical, there being an indentation in each to receive the ends of the steel wire. Some magnetism was then elicited by percussion in the larger rod, and the steel wire being properly placed between its upper extremity and the lower one of the small rod, the upper end of the latter was hammered, and magnetism thus communicated to the wire; whilst the lower rod, receiving some influence from the percussion, performed a similar office. The author calls this mode of proceeding, the *com-*

pound process, to distinguish it from the mere hammering of the wire upon the rod, as practised by him formerly, and which he terms the *simple process*. He then enters into extended details of his several experiments, of which the following are the principal results: first, that the *compound* process is more effectual in the production of magnetism than the *simple one*, though the ratio of augmentation does not appear determinate. In one experiment, the maximum effect of the simple process was an attractive force capable of lifting between 186 and 246 grains, while the *compound process* augmented the lifting power to 326 grains. In another, the *simple process* gave a lifting power of 246 grains, the *compound* of 345 grains. Moreover, the efficacy of the compound process is much less manifest upon long than short wires, and is greatly impaired by diminishing the size of the rods.

In respect to the influence of the temper of the wire upon the degree of magnetism developed, Mr. Scoresby found that the softer the wire the more susceptible it became of this magnetic condition.

The author concludes this paper with some theoretical remarks respecting the influence of percussion in disposing the particles of iron to acquire and retain magnetism, which he thinks may tend to explain some otherwise obscure phenomena; and which seem to render it probable that the process of percussion may be applied, in connexion with other modes of magnetizing, for giving increased power to magnets.

On Semi-decussation of the Optic Nerves. By William Hyde Wollaston, *M.D. V.P.R.S.* Read February 19, 1824. [*Phil. Trans.* 1824, *p.* 222.]

In the human brain, the optic nerves, after passing forward to a short distance from their origin in the thalami, become incorporated; and from the point of union two nerves are sent off, one to each eye. To this united portion the term Decussation has been applied, under the supposition that though the fibres do intermix, they still continue onward in their original direction; and that those from the right side cross over wholly to supply the left eye, while the right eye is similarly supplied by fibres from the left thalamus. Anatomists have considered this opinion as confirmed, by the circumstance of the nerves actually crossing each other as two perfectly distinct cords in some fish; the author, however, from a species of blindness under which he has more than once suffered, concludes that a different distribution of the nerves takes place in the human subject. This peculiar state of vision consisted in seeing only half of every object, the loss of sight being in both eyes towards the left, and of short duration only. In reflecting upon this subject, a certain arrangement of the optic nerves, not consistent with the generally received hypothesis of their decussation, occurred to him. Since the corresponding points of the two eyes, he observes, sympathize in disease,

their sympathy is evidently from structure, and not from mere habit of feeling together. Any two corresponding points must be supplied with a pair of filaments from the same nerve; and the seat of a disease in which similar parts of both eyes are affected, must be considered as situated at a distance from the eyes, at some place in the course of the nerves where these filaments are still united, and probably in one or other thalamus. It is plain, he continues, that the cord which comes finally to either eye, under the name of optic nerve, must be regarded as consisting of two portions, one half from the right thalamus, and the other from the left. Upon this supposition, decussation will take place only between the adjacent halves of the two nerves. That portion of nerve which proceeds from the thalamus to the right side of the right eye, passes to its destination without interference; and in a similar manner the left thalamus will supply the left side of the left eye with one part of its fibres, while the remaining halves of both nerves, in passing over to the eyes of the opposite sides, must intersect each other with or without intermixture of their fibres. The crossing of the entire nerves to the opposite eyes in fishes, Dr. Wollaston observes, is in conformity with this view of the arrangement of the human optic nerves; for in the sturgeon, for instance, the eyes are placed so exactly back to back, that there are no corresponding points of vision requiring to be supplied with fibres from the same nerve. In this animal, an injury to the left thalamus might be expected to occasion entire blindness of the right eye alone; in ourselves a similar injury would occasion blindness to all objects situated to our right, owing to insensibility of the left half of the retina of both eyes. Dr. Wollaston states some other facts, illustrating his view of this peculiar distribution of the human optic nerves, remarking that in common vision also the sympathy of corresponding points, which receive similar impressions from the same object, is dependent upon the same arrangement of nerves to which the term Semi-decussation may be applied. In conclusion, Dr. Wollaston remarks, that so long as our consideration of the functions of a pair of eyes is confined to the performance of healthy eyes in common vision, when we remark that only one impression is made upon the mind, though two images are formed on corresponding parts of the retina, we may rest satisfied in ascribing the apparent unity of the impression to habitual sympathy of the parts; but when we regard sympathy as arising from structure, and dependent on connexion of nervous fibres, we therein see a distinct origin of that habit, and have presented to us a manifest cause why infants first begin to give the corresponding direction to their eyes, and clearly gain a step in the solution, if not a full explanation, of the long-agitated question of single vision with two eyes.

Some curious Facts respecting the Walrus and Seal, discovered by the Examination of Specimens brought to England by the different Ships lately returned from the Polar Circle. By Sir Everard Home, *Bart. V.P.R.S. In a Letter addressed to Sir* Humphry Davy, *Bart. P.R.S.* Read March 4, 1824. [*Phil. Trans.* 1824, *p.* 233.]

The first fact stated by Sir Everard Home in this paper is the analogy in structure between the hind foot of the Walrus and the foot of the Fly. In both these is a very similar apparatus for producing a vacuum, so as to enable the animal to proceed upon smooth surfaces against gravity, by the adhesion of the feet thus effected; there being two cups in the foot of the fly and one in that of the walrus for this purpose. Secondly, he notices the peculiar mode in which the bile in the walrus is collected in a reservoir, and thence forcibly impelled into the duodenum.

The third new fact which the author adduces, is the peculiar structure of the funis and placenta of the Seal. In this animal the vessels forming the funis are not twisted; their whole length is nine inches; after passing three inches from the navel of the fœtus they give off anastomosing branches, connected with it by three membranous folds, between which the blood-vessels are conveyed to the placenta. This structure gives uncommon facility to the placental circulation, and makes it worth inquiry whether the same peculiarities exist in other marine animals.

Several illustrative drawings accompany this paper.

Additional Experiments and Observations on the Application of Electrical Combinations to the Preservation of the Copper Sheathing of Ships, and to other purposes. By Sir Humphry Davy, *Bart. P.R.S.* Read June 17, 1824. [*Phil. Trans.* 1824, *p.* 242.]

Since his former communication the President has had an opportunity of prosecuting his researches upon the above subjects, upon an extended scale, and with results perfectly conclusive and satisfactory. He found that sheets of copper defended by from one 100th to one 150th part of zinc or iron, exposed for many weeks to the full flow of the tide in Portsmouth harbour, suffered no corrosion, and that even one 1000th part of cast iron exerted great protecting influence. Boats and the sides of ships protected in this way were also similarly preserved. Of the different protecting metals cast iron is most convenient, and the plumbaginous substance formed upon it does not impede the electrical action. The President formerly anticipated the deposition of earthy substances upon the negative copper, and this he now found to take place upon sheets of copper exposed about four months to sea-water, and defended by from one 50th to one 80th their surface of zinc and iron. They became coated with carbonate of lime and magnesia; but this effect is easily prevented by duly diminishing the proportion of the protecting metal, so as to prevent the excess of negative power in the copper, which then remains bright and clean.

The author observes, that many singular facts have occurred in the course of his researches, some of which bear upon general science. Weak solutions of salt act strongly upon copper, but strong ones do not affect it, apparently because they contain little air, the oxygen of which seems necessary to give the electro-positive power to these menstrua; upon the same principle, alkaline solutions and lime-water prevent the action of sea-water on copper, having in themselves the positive electrical energy which renders the copper negative.

The author concludes this paper with some further applications of electro-chemical theory to the subject of it, and refers to the principles developed, as suggesting means of preserving instruments of brass and of steel, by iron and by zinc,—a circumstance already taken advantage of by Mr. Pepys, in inclosing delicate cutting instruments in handles or cases lined with zinc.

On the apparent Direction of Eyes in a Portrait. By William Hyde Wollaston, *M.D. F.R.S. and V.P.* Read May 27, 1824. [*Phil. Trans.* 1824, *p.* 247.]

When we consider, says the author, the precision with which we commonly judge whether the eyes of another person are fixed upon ourselves, it is surprising that the grounds of such judgement are not distinctly known, and that most persons in attempting to explain the subject would overlook some of the circumstances by which they are generally guided. Though it may not be possible to demonstrate, by any decisive experiment, on the eyes of living persons what those circumstances are, we may find convincing arguments to prove their influence, if it can be shown in the case of portraits, that the same ready decision that we pronounce on the direction of the eyes is founded, in great measure, on the view presented to us of parts which have not been considered as assisting our judgement.

Dr. Wollaston then adverts to the influence of the form of the iris, as announcing the direction of the eye in portraits, and to that of the variable portion of the white shown when the eye is variously directed in living persons: he remarks, however, that even in real eyes we are not guided by this circumstance alone, but are unconsciously aided by the concurrent position of the face; and he illustrates this opinion by reference to a series of drawings annexed to the paper, and which show that the apparent position of the eyes is principally influenced by that of the adjacent parts of the face, especially those which are most prominent; and these considerations are not limited in their application merely to cases of lateral turn of the eyes and face. But the same principles also apply to instances of moderate inclination of the face upwards or downwards; for when the face is directed downwards, the eyes that look at us must be turned upwards, from the position of the face to which they belong; and if to eyes so drawn an upward cast of features be substituted for the former, the eyes immediately look above us, as is found by a sketch

annexed. From these and other details given in the paper, the author concludes that the apparent direction of the eyes to or from the spectator, depends upon the balance of two circumstances combined in the same representation; namely, 1st, the general position of the face presented to the spectator; 2nd, the turn of the eyes from that position; and thence proceeds to examine why, if the eyes of a portrait look at the spectator placed in front of the picture, they appear to follow him in every other direction. When two objects are seen on the ground at different distances from us in the same direction, one appears and must be represented exactly above the other, so that a vertical plane from the eye would pass through them; and since such a line will be seen upright, however far we remove to one side, it follows that the same objects still seem to be in a line with us exactly as in the front view, seeming as we move to turn from their first direction.

In portraits the permanence of direction, with reference to the spectator, and corresponding change of its apparent position in space when he moves to either side, depends upon the same principles. The nose drawn in front, with its central line upright, continues directed to the spectator, though viewed obliquely; or if the right side of the nose is represented, it must appear directed to the right of the spectator in all situations; so that eyes that turn in a due degree from that direction towards the spectator, so as to look at him when viewed in front, will continue to do so when viewed obliquely.

Further Particulars of a Case of Pneumato-thorax. By John Davy, *M.D. F.R.S.* Read March 4, 1824. [*Phil. Trans.* 1824, *p.* 257.]

About a month after the operation described in Dr. Davy's former paper, when the patient appeared to be doing well, symptoms of hydrothorax came on, and fluid again collected in the left side of the chest. A second operation therefore was performed, and fourteen ounces of fluid discharged through a perforation in the fifth rib. During the six following weeks not less than twenty pints of fluid ran off through the opening; at first it was transparent, but became gradually more and more purulent, and was mixed with air composed of oxygen, azote and carbonic acid, in various proportions. The patient's health improved at first progressively, but in about six weeks after the operation he became worse, and expired suddenly. On examination after death, about six ounces of pus were found in the left pleura. The right pleura was healthy, but tubercles and vomicæ were found in the right lung; the left lung was much condensed, and communicated by two small openings with the pleura. Dr. Davy refers the origin of the disease in this case to a communication between the aspera arteria and cavity of the pleura, established by the rupture of a superficial bronchial tube, and the membrane covering it; and concludes the paper with some remarks upon the fluctuating composition of the air from the chest, which he attributes not to the varying quantity of atmospheric air, admitted through the perfora-

tion, which was as carefully closed as possible, but to its vitiation by respiration, and by the absorbent power of the pleura.

On the Action of finely divided Platinum on Gaseous Mixtures, and its Application to their Analysis. By William Henry, *M.D. F.R.S.* Read June 17, 1824. [*Phil. Trans.* 1824, *p.* 266.]

In the first section of this paper the author describes the action of finely divided platinum, at common temperatures, on mixtures of hydrogen and olefiant gas with oxygen; of hydrogen and carburetted hydrogen with oxygen; of hydrogen and carbonic oxide with oxygen; of hydrogen and cyanogen with oxygen; of carbonic oxide and carburetted hydrogen with oxygen; of hydrogen, carburetted hydrogen and carbonic oxide with oxygen; and of the same with the addition of olefiant gas. From the experiments detailed under these several heads, it appears that when the compound combustible gases mixed with each other, with hydrogen, and with oxygen, are exposed to balls of platinum sponge, the several gases are not acted upon with equal facility; but that next to hydrogen, carbonic oxide is most disposed to unite with oxygen, then olefiant gas, and lastly carburetted hydrogen. By due regulation of the proportion of hydrogen, the author remarks, that it is possible to change the whole of the carbonic oxide into carbonic acid, without acting on the olefiant gas or carburetted hydrogen; he observes, however, that with respect to olefiant gas this exclusion is attended with some difficulty, and it is generally more or less converted into carbonic acid and water.

The second section of this paper relates to the action of finely divided platinum upon gaseous mixtures at increased temperatures. In these experiments the gases, mixed with oxygen enough to saturate them, were severally exposed in small retorts containing a platinum sponge, and immersed in a mercurial bath, to a temperature which was gradually raised till the gases began to act on each other. It was thus found that carbonic oxide began to be converted into carbonic acid at about 300°; olefiant gas was decomposed at about 500°; carburetted hydrogen at a little above 555°; and cyanogen appeared to require a red heat.

Muriatic acid, mixed with half its volume of oxygen, began to be acted upon at 250°, and ammoniacal, with an equal volume of oxygen, at 380°.

Adverting to the property inherent in certain gases of retarding the action of the platinum, when they are added to explosive mixtures of oxygen and hydrogen, Dr. Henry observes, that it is most remarkable in those which possess the strongest attraction for oxygen; and that it is probably to the degree of this attraction, rather than any agency arising out of their relations to caloric, that we are to ascribe the various powers which the gases manifest in this respect.

From his experiments on the action of the platinum on mixed gases, at high temperatures, the author was led to the following

mode of procuring pure carburetted hydrogen. The early product of the distillation of pit-coal was washed with a solution of chlorine, and afterwards with liquid potash. The residue was then mixed with one fourth its volume of oxygen, and heated to 350°, in contact with the platinum, which converted the carbonic oxide into acid, and the hydrogen into water. The carbonic acid being removed by liquid potash, there remained only the carburetted hydrogen, the redundant oxygen, and a trace of nitrogen.

Dr. Henry concludes this communication by pointing out the best method of applying the facts detailed in the preceding sections to the analysis of mixtures of the combustible gases in unknown proportions.

A Comparison of Barometrical Measurement, with the Trigonometrical Determination of a Height at Spitzbergen. By Captain Edward Sabine, *of the Royal Regiment of Artillery, F.R.S.* Read May 6, 1824. [*Phil. Trans.* 1824, *p.* 290.]

The hill selected for this comparative measurement was the highest within convenient distance, of which the ascent was practicable, on the western part of the north coast of Spitzbergen. The summit was less than two miles from the observatory, in a direction nearly due south, the observatory being upon an island rather more than a mile from the main land. In consequence of the extreme inaccuracy of the chart of Fair Haven, published in Captain Phipps's voyage, the author has annexed to this paper a sketch of the harbour and adjacent coast, to show the positions of the hill and observatory. The small bay formed by the shore of the main land, to the north-east end of the hill, being frozen over, afforded a perfectly level base, and corrections for inequality were thus rendered unnecessary. A polished copper cone was fixed upon a staff at the summit of the hill, the apex of which was proposed as the height to be measured: it stood 44 inches above the highest pinnacle of the summit. Captain Sabine then enters into the details of this trigonometrical measurement, from which the altitude of the cone is considered as equal to 1644 feet. The author next proceeds to detail the particulars of the barometrical measurement, and the precautions taken to insure accuracy in the instruments, and in their employment; and the height of the cone thus ascertained was 1640·07 feet.

Captain Sabine concludes this paper with some remarks upon the incorrectness with which the heights of the hills on this coast are set down in Captain Phipps's voyage.

Experimental Inquiries relative to the Distribution and Changes of the Magnetic Intensity in Ships of War. By George Harvey, *Esq.* *Communicated by* John Barrow, *Esq. F.R.S.* Read Feb. 26, 1824. [*Phil. Trans.* 1824, *p.* 310.]

This paper contains the details of experiments made on board several vessels, with a view of determining the influence of the iron in the ships upon the compass under different circumstances and situations. The instrument used for determining the intensity consisted of a magnetized cylindrical bar, 2·5 inches long and three eightieths of an inch diameter, delicately suspended by a single fibre of the silkworm, to the extremity of an adjusting screw, which worked in the cap of the glass vessel inclosing the bar. A brass wire also passed through the cap for the purpose of placing the bar at right angles to the magnetic meridian previous to its being put into a state of oscillation.

On the days devoted to the experiments on ship-board, the time of making 50 vibrations of the bar was determined in the centre of a meadow, of which the substratum was dry slate, by a mean of six sets of experiments, the time being accurately registered to quarter seconds. The instrument was then taken on board, and placed in succession at the different stations of the ship, and the mean of six sets of experiments determined at each station with the same precautions as on land. The times, says the author, of performing the oscillations on shore, and at each of the assumed points in the ship, necessarily gave the magnetic intensity at each station in terms of the terrestrial intensity, which in this case was represented by 100.

Experiments on the Elasticity and Strength of Hard and Soft Steel. In a Letter to Thomas Young, *M.D. For. Sec. R.S.* *By Mr.* Thomas Tredgold, *Civil Engineer.* Read March 25, 1824. [*Phil. Trans.* 1824, *p.* 354.]

The bars of steel used in these experiments were supported at the ends by two blocks of cast iron, resting upon a wooden frame, and a scale for weights was suspended from the middle of the length of the bar, by a cylindrical steel pin, three eighths of an inch in diameter. To measure the flexure a quadrantal piece of mahogany was attached to the frame, with a vertical bar sliding in two guides at its edge, and moving an index. The bar and index were so balanced, that one end of the bar bore with constant pressure upon the specimen, and the graduated arc was divided into inches, tenths, and hundredths. The thousandths were measured by a vernier. A bar of blistered steel of file hardness, 13 inches long between the supports, underwent no permanent alteration of form when loaded with 110lbs. The temper of the bar was then successively lowered, and it was ultimately again hardened; but in these different states its flexure and resistance to permanent change of form remained the same.

These experiments were repeated with bars of other dimensions, which were loaded till they broke; and from them the author also infers that the elastic force of steel is not altered by temper, and that the force which produces permanent alteration is to that which causes fracture in hard steel, as 1 : 1·66; and in the same steel of a straw yellow temper, as 1 : 2·56. From comparisons of the strain required to cause permanent alteration in different kinds of steel, the author concludes, that in the process of hardening, the particles are put into a state of tension among themselves, which lessens their power to resist extraneous force; and the phenomena of hardening may be referred to the more rapid abstraction of heat from the surface of the metal than can be supplied from the internal parts, whence a contraction of the superficial parts round the expanded central ones, and a subsequent shrinking of the latter, by which the state of tension is produced.

A short Account of some Observations made with Chronometers, in two Expeditions sent out by the Admiralty, at the recommendation of the Board of Longitude, for ascertaining the Longitude of Madeira and of Falmouth. In a Letter to Thomas Young, *M.D. For. Sec. R.S. and Secretary to the Board of Longitude. By Dr.* John Lewis Tiarks. Read April 29, 1824. [*Phil. Trans.* 1824, *p.* 360.]

Dr. Tiarks was sent to Madeira in the year 1822 with 15 chronometers, of which the rates had principally been ascertained in the Royal Observatory of Greenwich; he touched at Falmouth both in going out and returning; and having again ascertained the rates of his time-keepers, he was thus enabled to obtain two distinct determinations of the longitude of Falmouth, which differed about four seconds of time from that which had been inferred from the Trigonometrical Survey of Great Britain. It became therefore desirable that some further operations should be undertaken for the removal or elucidation of this discordance; and the following year a similar method was adopted with 25 chronometers, for determining the difference of longitude between Falmouth and Dover; this latter station having been chosen as easy of access, and as being perfectly determined; and the computations were made by interpolation, without employing any other rates for the chronometers than those which were observed in the different trips while they were actually on board the ship; and latterly, when Dover Roads became unsafe, the operations were limited to the distance from Portsmouth to Falmouth: thus, between the months of July and September, the observations were made three times at Dover, four times at Falmouth, and three times at Portsmouth; and the comparison of their results affords a correction of five seconds of time for the difference of longitude of Dover and Falmouth, and of three for the difference of Falmouth and Portsmouth, agreeing completely with the error of four seconds, attributed from the observations of the preceding year to the difference of longitude of Falmouth and Greenwich.

Hence Dr. Tiarks thinks it fair to conclude that the diameter of the parallel circle, in which the longitude is measured, has in the survey been taken somewhat too great, and consequently the earth's ellipticity greater than the truth. He remarks that the measurement of the spheroidal triangle concerned, determines only the actual flatness of the part of the earth's surface on which it is situated, and not the actual magnitude of the whole parallel, unless its curvature be supposed perfectly uniform, which we cannot assume with confidence; while on the other hand, if we compute the ellipticity from the result of the chronometrical determination, it becomes one 314th instead of one 150th, and agrees with the most accurate measurements obtained from different principles. The longitude of Falmouth is finally determined to be $20^{m}\ 11^{s}{\cdot}1$ of time, and that of the British Consul's garden at Funchal, $1^{h}\ 7^{m}\ 39^{s}$ W. of Greenwich.

Of the Effects of the Density of Air on the Rates of Chronometers. By George Harvey, *F.R.S.E. &c. Communicated by* Davies Gilbert, *Esq. V.P.R.S.* Read May 13, 1824. [*Phil. Trans.* 1824, *p.* 372.]

Among the sources of error to which chronometers are liable, the effect of the variable density of the medium in which the balance vibrates has been overlooked; the author therefore proposes to investigate the effects of diminished and increased pressure of transference from one to the other, and of the ordinary variations of atmospheric density upon the rates of chronometers. In respect to diminished pressure, he found that chronometers gained by being placed in air of less density than that of the ordinary state of the atmosphere, and that, on the other hand, they lost when subjected to air of greater than ordinary density. These experiments were made with a variety of chronometers, placed in the receiver of an air-pump, or in that of a condensing apparatus.

In respect to the influence of ordinary changes in the density of the air, the author remarks that pocket chronometers are more readily affected than box chronometers, but that they all exhibit an increased rate under diminished density, and *vice versâ*. The author shows that these changes in the rates, as observed in the air-pump and condensing apparatus, are independent of the changes of temperature, resulting from changes in the density of the air thus rapidly effected, and therefore proceeds to inquire into the actual cause of the changes which his experiments indicate; he refers them to an increase in the arc of vibration when the density is diminished, and to a diminution in the arc under increased density.

rr from Lewis Weston Dillwyn, *Esq., addressed to Sir* Humphry *y, Bart. P.R.S.* Read March 25, 1824. [*Phil. Trans.* 1824, 13.]

letter is supplementary to the former one, and contains observations on the relative periods at which the different fa- of testaceous animals appear to have been created, and on dual approximation, which may be observed in British strata, ie fossil remains of the oldest formations to the living inhabi- f our present land and waters.

author observes, that the dimyairia of the strata between the ion lime and lias have the ligament external, and that internal nts were therefore confined to the monomyairia till after the ion of the lias. In the beds above the lias all the shells are ile to existing orders of animals, and it is only in the tertiary iat any of the cirrhipeda or families of naked mollusca have ound.

at is generally considered as the beak of a sepia, Mr. Dillwyn to the cephalopode animal of an ammonite. Every shell of the y strata, the author observes, may be referred to some existing ; but though this approximation has thus far proceeded in the n clay, yet its numerous species are now extinct, and it is only upper beds of crag that any fossil can be completely identified living species.

count of the Organs of Generation of the Mexican Proteus, ed by the Natives Axolotl. By Sir Everard Home, *Bart. P.R.S.* Read June 17, 1824. [*Phil. Trans.* 1824, *p.* 419.]

specimens described in this paper were taken in the month of in a lake three miles from Mexico, at an elevation of 8000 feet the level of the sea. The usual temperature of the lake is 60°, ey are in such abundance as to form a principal article of food peasantry. By the assistance of annexed drawings by Mr. , Sir Everard fully describes the male and female organs of animals, and is enabled to decide that they are a full grown erfect tribe. "The attack therefore," says the author, "made Mr. John Hunter's sagacity by Mr. Rusconi, in his work *sur ours des Salamandres Aquatiques*, retorts upon himself."

count of Experiments on the Velocity of Sound, made in Hol- l. By Dr. G. Moll, *Professor of Natural Philosophy in the versity of Utrecht, and Dr.* A. Van Beek. Read March 18, 4. [*Phil. Trans.* 1824, *p.* 424.]

er adverting to the difference between the celerity of sound, as ed by theory and found by experiment; and to Laplace's ex- ion of the cause of that difference, and his corrections of the onian formula,—the authors proceed to consider the influence of

the variable force of wind upon its velocity, and state their mode of annihilating such cause of error. They then detail their own experiments, for which they selected two open and elevated spots in the plains of Utrecht, distinctly visible from each other, and distant about 9664 fathoms. They measured the interval between seeing the light and hearing the sound by clocks with conical pendulums, which divide the 24 hours into 10,000,000 parts, and one of the indexes of which gives one 100th part of a decimal second. Each station was also furnished with a good barometer, several accurate thermometers and excellent telescopes, and the humidity of the air was determined by Daniell's hygrometer. The authors then describe the means which they adopted to insure the simultaneous firing of shots at both stations, and by which they succeeded in bringing them within 1″ or 2″ of each other, and enter at considerable length into the details of their different experiments, the results of which are given in several tables annexed to this paper; among which will be found one exhibiting a general view of the results of the experiments of those different philosophers who have investigated this subject.

In conclusion, it appears from their researches that at the temperature of 32°, the velocity of sound is 1089·7445 English feet per sexagesimal second.

A Catalogue of nearly all the principal fixed Stars between the Zenith of Cape Town, Cape of Good Hope, and the South Pole, reduced to the 1*st of January*, 1824. *By the Reverend* Fearon Fallows, *M.A. F.R.S.* Read February 26, 1824. [*Phil. Trans.* 1824, *p.* 457.]

Remarks on the Parallax of α Lyræ. By J. Brinkley, *D.D. F.R.S. &c. Andrews Professor of Astronomy in the University of Dublin.* Read March 11, 1824. [*Phil. Trans.* 1824, *p.* 471.]

The author's object in this paper is principally to form a correct estimate of the absolute and relative degrees of accuracy of the instruments at Dublin and at Greenwich. He first considers the difference of parallax between γ Draconis and α Lyræ, and secondly, the absolute parallax of α Lyræ.

He exhibits, in a table, the whole of the results of 337 observations of Mr. Pond for the intercepted arc, reduced to the 1st of January 1815, chiefly by Mr. Pond's own computations. From 46 observations made in the year 1812, he deduces 0″·28 for the coefficient of the effect of parallax; and from such of his observations as were made in the same day, the number deduced is 0″·54.

In 1813 there is a difference of half a second between the mean of 22 observations in June and July, and of 17 in August; hence Dr. Brinkley was led to examine the observations of this year alone, and he found that 61 of them, from June to December, as reduced by Mr. Pond, gave 0″·42 for the coefficient of parallax; and omitting the last five days of observation 0″·89, which is little less than the result of his own researches.

On the other hand, when five double observations, in January and ebruary 1814, were added to these 61, they reduced the result for e coefficient to $0''\cdot18$; so that the discordancies seem to be too great enable us to place any reliance on the conclusions respecting the tual magnitude of the annual parallax.

A similar fluctuation is observable in the results obtained for the llowing years; and though it might, on the whole, be inferred that e parallax is about three fourths as great as that which the author s assigned from his own observations, yet he contents himself with ncluding that the mural circle of Greenwich has not sufficiently oved the identity of the distance of the two stars in summer and inter, within one tenth of a second; but, on the contrary, that it ows the parallax of α Lyræ to be half a second greater than that γ Draconis.

In 1815 the first 15 summer observations, compared with the first 3 in November, give a parallax of $+ 0''\cdot72$; the next 16 in summer, mpared with the next 16 in winter, give a negative parallax of $0''\cdot58$; a comparison which sufficiently proves the imperfection of e observations, depending probably on an unsteadiness in the inrument.

In the whole five years, the mean of all the observations in August ceeds the mean of July by $0''\cdot51$, a discordance which parallax ould diminish but in an inconsiderable degree.

The author pursues a similar train of argument in the second part the inquiry, relating to the absolute parallax of α Lyræ. While e circle at Dublin, he observes, made from a mean of several years e double zenith distance of this star $3''$ greater in the beginning of ecember than in the beginning of August, that of Greenwich shows difference whatever in the double altitude observed by reflection summer and winter. There are, however, differences of about ur seconds in the difference of altitude of α Lyræ and the pole star, determined in different years by the same instrument; and Dr. rinkley observes, that an unsteadiness, amounting to $15''$ or $20''$, is scoverable in the comparative results of the different microscopes; hence he infers that there must be an uncertainty, amounting to any tenths of a second in the mean.

The coefficients of aberration and of solar nutation, which come it $20''\cdot35$ and $0''\cdot51$, are certainly true to one fourth or one tenth a second, as deduced from the observations of Dublin; the author inks it fair, therefore, to infer that $1''\cdot14$, the coefficient for annual arallax for α Lyræ, is correct nearly in the same proportion. Nor e there any changes from season that could produce the appearance regular parallax of all the stars of which it has been inferred; and is very improbable that any error of the instrument could have given parallax to α Lyræ, and left the pole star completely free from it.

The last of the tables shows the consistency of the circle of Dublin the places of the stars, as determined by it after the interval of a nsiderable number of years, without any such tendency to the south is supposed to have been observed at Greenwich.

Observations of the apparent Distances and Positions of 380 *Double and Triple Stars, made in the Years* 1821, 1822, *and* 1823, *and compared with those of other Astronomers; together with an Account of such Changes as appear to have taken place in them since their first Discovery. Also a Description of a Five-feet Equatorial Instrument employed in the Observations. By* John Frederick William Herschel, *Esq. F.R.S. and* James South, *Esq. F.R.S.* Read January 15, 1824. [*Phil. Trans.* 1824, *p.* 1.]

The determination of the apparent distances and positions of such double stars as could be measured with micrometrical instruments and high magnifying powers, was suggested by Sir William Herschel more than forty years ago, and in his hands it led to a new department of physical astronomy, by the discovery of sidereal phenomena referrible to the agency of attractive forces; but the determination of the existence of annual parallax, the immediate object for which the inquiry was instituted, was soon lost sight of in the more extensive views of the construction of the universe, which gradually unfolded themselves. Nor has the investigation been resumed, although from the precision with which such observations can be made, it seems, in the opinion of the authors of this paper, likely to be the mode by which the existence or non-existence of sensible parallax will ultimately be determined.

The results of Sir William Herschel's observations, from 1779 to 1784, were published in the Philosophical Transactions from 1782 to 1785; and a re-examination, after a lapse of twenty years, was undertaken by him in 1801, -2, -3, and -4; and in the Transactions for 1802 and 1804, unexpected phenomena were communicated. Instances in which two stars were performing to each other the offices of sun and planet were proved to exist; and to more than one pair the period of rotation was, according to the observations of the authors of this paper, ascertained with considerable exactness. Immersions and emersions of stars behind each other had been witnessed; and real motions among some of them had been observed rapid enough to be detected in very short intervals of time.

But as from the novelty of the subject, and from the imperfections of the micrometers employed in 1779 and 1780, it was likely that some instances of error had occasionally crept in, it became desirable that a second re-examination should be instituted:—accordingly, in the year 1816, some progress was made by Mr. Herschel towards its accomplishment, and the results are communicated in the present paper. A similar idea having likewise occurred to Mr. South, it was at length determined that the observations should be carried on in concert, and with his instruments.

Meanwhile (unknown to the authors of this paper) a similar undertaking had been entered upon by a distinguished continental astronomer, Mr. Struve, Director of the Imperial Observatory at Dorpat; and the general coincidence between the measures of this observer and those of their own, is deemed at once interesting and corroborative of the accuracy of both.

.The instruments with which the observations accompanying this aper were made, are a five-, and a seven-feet equatorial; the former ras constructed under the direction of the late Capt. Huddart, and remarkable for its extreme lightness, for the promptitude with rhich it obeys its adjustments, and for its ability in retaining them. ts object-glass, of 3¾ inches aperture, and of five-feet focal length, the work of the late P. and J. Dollond; whilst its divided circles, icroscopes, &c., were completed by Messrs. J. and E. Troughton. description of it is given, and a drawing is annexed. The latter a telescope of 7-feet focal length, and five inches clear aperture; was made by Tulley, and is mounted on the polar axis of the old quatorial sector, made by Sisson for the Royal Observatory, and r the use of which, acknowledgment is made to the Council of this ociety.

The micrometers employed are the work of Mr. Troughton, and ave long since been familiar to astronomers under the name of 'roughton's Wire Micrometer. The measures of distance are all entral. Various precautions employed in conducting the observa-ons are narrated; contrivances whereby some difficulties were sur-ounted are enumerated. The observations of each star were ge-erally made in each other's presence, but occasionally in different arts of the observatory, and with different instruments, without any ommunication with each other. In some instances the observations f Mr. Troughton or Mr. Richardson have been appealed to, in order settle discrepancies.

To the observations of each star the authors attach their mean esult; the results obtained by other observers are also placed in the rder in which they were made; but there is one circumstance to rhich they solicit attention, namely, that as far as Sir William Her-hel's observations are concerned, the dates and results will not ac-ord with those published by Sir William in the Transactions, for easons which will be found in a former part of the paper.

As an appendix, measures of a few stars less perfectly observed are dded, which, although not entitled to equal confidence with the thers, the authors think may perhaps still have their use.

n the Effects of Temperature on the Intensity of Magnetic Forces; and on the Diurnal Variation of the Terrestrial Magnetic Intensity. By Samuel Hunter Christie, *Esq. M.A. of Trinity College, Cambridge, Fellow of the Cambridge Philosophical Society: of the Royal Military Academy. Communicated by the President.* Read June 17, 1824. [*Phil. Trans.* 1825, *p.* 1.]

The details of the author's experiments upon the above subjects re given in an extended series of tables, commencing with a tempe-ature of — 3° Fahr. up to 127° Fahr. Mr. Christie found that as e temperature of the magnets increased, their intensity diminished, direct contradiction to the notion of destroying magnetism by in-ense cold. From a temperature of 80° the intensity decreased ra-

pidly as the temperature increased, and at above 100° a portion of the power of the magnet was permanently destroyed.

In regard to the diurnal changes in the terrestrial intensity, the author's experiments lead him to suggest the following queries for the consideration of those who may have an opportunity of making such observations:—Does the time of the minimum intensity correspond with the time at which the sun is on the magnetic meridian? Does the time of maximum intensity correspond to the sun's passing the plane of the equator of the dipping-needle? Does any change take place in the intensity while the sun is below the horizon? Are any periodical effects corresponding to the time of rotation of the sun about its axis observable? Is the diurnal change of intensity at the time of new moon sensibly different from what it is at the time of quadrature? If the moon do produce an effect on the needle, it is evidently less than that of the sun;—should we then attribute it to solar heat, or to the magnetism of the sun?

The Croonian Lecture. On the Existence of Nerves in the Placenta. By Sir Everard Home, *Bart. V.P.R.S.* Read November 18, 1824. [*Phil. Trans.* 1825, *p.* 66.]

In this lecture the author makes known his discovery of the existence of nerves, both in the fœtal and maternal portions of the placenta. His previous researches had led him to doubt the existence of blood-vessels without nerves, and the extreme vascularity of the placenta led him to suspect them in that organ. With the assistance of Mr. Bauer, therefore, he first examined the placenta of the Seal, the arteries and veins of which had been injected, and in which nerves were discovered, not only surrounding the umbilical arteries, but also in the uterine portion.

In the pregnant uterus of the Tapir of Sumatra, in which, there being no placenta, the umbilical chord is connected with the chorion, the nerves were very conspicuous in the transparent portion of the chorion, along which the branches of the funis pass before they arrive at the spongy part.

Having thus established the existence of nerves in the placenta, and where that is wanting in the flocculent chorion, Sir Everard proceeds to offer some general remarks upon their probable uses and influences.

From the various sources, the number, and the ganglia of the uterine nerves, and from the circumstance of their becoming enlarged during pregnancy, he infers their powerful influence on the fœtus in utero; and for the further illustration of this subject, the author adds a description of the nerves connected with the generative organs in the human species, the quadruped, the bird, and the frog.

He concludes this lecture with remarking, that since the discovery of the placental nerves proves the existence of a communication through their medium, between the brain of the child and that of the mother, some light may be thrown on the degree of dependence

in which the fœtus is kept during the whole time of utero-gestation, and upon the influence of the bodily and mental affections of the mother upon the child; in further illustration of which, several instances are detailed in proof of the descent of various peculiarities of the mother to the offspring.

Observations on the Changes the Ovum of the Frog undergoes during the Formation of the Tadpole. By Sir Everard Home, *Bart. V.P.R.S.* Read November 25, 1824. [*Phil. Trans.* 1825, *p.* 81.]

The ova of the Frog, when examined in the ovaria, consist of dark coloured vesicles, which acquire a gelatinous covering on entering the oviduct, and are completely formed by the time they reach the cavities in which the oviducts terminate, and during their expulsion from which they receive the male influence; after this, the contents of the ovum, previously fluid, coagulate and expand, the central part being converted into brain and spinal marrow, while in the darker substance of the egg the heart and other viscera are formed. The membrane forming the vesicles being destined to contain the embryo when it has become a tadpole, enlarges as the embryo increases, and may be said to perform the office both of the shell and its lining membrane in the pullet's egg, serving as defence and allowing aëration. The black matter which lines the vesicle probably tends to the defence of the young animals from the too powerful influence of the solar rays, frogs' spawn being generally deposited in exposed situations. Sir Everard observes, that in the aquatic Salamander, an animal whose mode of breeding closely resembles the frog, this nigrum pigmentum is wanting; but that that animal deposits its eggs within the twisted leaves of water plants, which afford them an equivalent protection.

A general Method of calculating the Angles made by any Planes of Crystals, and the Laws according to which they are formed. By the Rev. W. Whewell, *F.R.S. Fellow of Trinity College, Cambridge.* Read November 25, 1824. [*Phil. Trans.* 1825, *p.* 87.]

The author, after stating the inconsistencies, inelegancies, and imperfections of the received notation for expressing the planes of a crystal, and the laws of decrement by which they arise, and of the usual methods of calculating their angles, explains the object of the present paper, which is to propose a system exempt from these inconveniencies, and adapted to reduce the mathematical portion of crystallography to a small number of simple formulæ, of universal application. According to the method here followed, each plane of a crystal is represented by a symbol indicative of the laws from which it results, which, by varying only its indices, may be made to represent any law whatever; and by means of these indices, and of the primary angles of the substance, we may derive a general formula expressing the dihedral angle contained between *any one plane* re-

sulting from crystalline laws, and *any other*. In the same manner we can find the angle contained between any two edges of the derived crystal. Conversely, having given the plane, or dihedral angles of any crystal and its primary form, we can, by a direct and general process, deduce the laws of decrement according to which it is constituted.

The purely mathematical part of this paper depends on two formulæ, demonstrated by the author elsewhere and here assumed as known; by means of one of which the dihedral angle included between any two planes can be calculated, when the equations of both planes are given; and by the other, the plane angle included between any two given right lines can in like manner be expressed by assigned functions of the coefficients of their equations, supposed given. These formulæ being taken for granted, nothing remains but to express by algebraical equations the planes which result from any assigned laws of decrement, for the different primitive forms which occur in crystallography.

To this effect, the author assumes one of the angles of the primitive form, supposed, in the first case, a rhomboid, as the origin of three coordinates, respectively parallel to its edges, and supposes any secondary face to arise from a decrement on this angle, by the subtraction of any number of molecules on each of the three edges. It is demonstrated first, that the equation of the plane arising from this decrement will be such, that the coefficients of the three coordinates in it (when reduced to its simplest form,) will be the reciprocals of the number of molecules subtracted on the edges to which they correspond. If the constant part of this equation be zero, the face will pass through the origin of the coordinates; if not, a face parallel to it may be conceived passing through such origin, and will have the same angles of incidence, &c. on all the other faces of the crystal; so that all our reasonings may be confined to planes passing through the origin of the coordinates.

To represent any face, the author incloses between parentheses the reciprocal co-efficients of the three coordinates of its equation, or rather of the numbers of molecules subtracted on each of the three edges to form it, with semicolons between: this he calls the symbol of that face. He then shows how truncations on all the different edges and angles of the primitive form are represented in this notation, by one or more of the elements of which the symbol consists becoming zero or negative, thus comprehending all cases which can occur in one uniform analysis.

The law of symmetry in crystallography requires that similar angles and edges of the primitive form should be modified similarly to form a perfect secondary crystal. This gives rise to *co-existent planes*. In the rhomboid, these co-existent planes are found by simple permutation of the elements of the symbol one among another. In the prism, such only must be permuted as relate to similar edges. In other primitive forms, as for example in the tetrahedron, the author institutes a particular inquiry into the decrements of co-existent

planes, which truncate the different angles of the primitive form, as referred to that particular angle which he assumes as the origin of the coordinates. It follows from this example, that in this latter case each of the elements of the symbol must be combined with its excess over each of the remaining two, to form a new symbol. This gives four symbols (including the original one), each susceptible of six permutations, making in all 24 faces.

The author then proceeds to consider the cases of the irregular tetrahedron and octohedron, the triangular prism, and rhomb dodecahedron, investigating in each case the symbols of the co-existent planes, and illustrating his theory with examples taken from the crystalline forms of zircon, sulphur, and other minerals. He next treats of the order in which the faces lie in a perfect crystal, and the determination of such faces as are adjacent or otherwise. To this end, he conceives an ellipsoid inscribed within the crystal, having for its three axes the three most remarkable lines in the primitive form, and by means of the well-known equation of the second degree representing such an ellipsoid, combined with the equation of any proposed, he deduces the longitude and latitude, on the surface of the ellipsoid, of the point at which it would be touched by a plane parallel to such face. The results are included in general and explicit formulæ, by whose application, in any proposed case, the sequence and arrangement of the faces in the perfect crystal are readily discovered.

The angles made by edges of the secondary form are next investigated; after which the author, having recapitulated his results, takes occasion to refer to a paper by Mr. Levy, who had previously, but unknown to Mr. Whewell, employed the representation of a secondary plane, by its equation referred to the three principal edges of the primitive form, but only in a particular case; whereas the investigation and notation in the present paper are absolutely general.

In the course of this paper, Mr. Whewell instances the application of his analysis to the solution of the following problems:—

Knowing the dihedral angles of the secondary rhomboid, to find the symbol of its faces, or their laws of decrement.

To find what laws of decrement give a secondary rhomboid similar to the primary one.

Knowing the lateral angles made by the planes of any bipyramidal dodecahedron, to find the symbols.

Knowing the angles made by any plane, with two primary planes to find its symbol.

To find what laws give prisms parallel to the axis of the rhomboid.

To find the symbol of a plane which truncates the edge of any secondary rhomboid.

Explanation of an Optical Deception in the Appearance of the Spokes of a Wheel seen through vertical Apertures. By P. M. Roget, M.D. F.R.S. Read December 9, 1824. [*Phil. Trans.* 1825, *p.* 131.]

The optical deception which Dr. Roget describes, takes place when a carriage-wheel, rolling along the ground, is viewed through the intervals of a series of vertical bars, such as those of a palisade, or of a Venetian window-blind. In those circumstances, all the spokes appear curved, those which are situated vertically excepted, the degree of curvature increasing as their position recedes from the vertical, and being a maximum for the horizontal spokes. The convexity of these curved images is always turned downwards, and the direction of their curvature is the same whichever way the wheel be moving.

To determine the influence of variations of circumstances on these phenomena, appeared to Dr. Roget the proper mode of obtaining a clue to their explanation. It results from his experiments, that a certain degree of velocity in the wheel is necessary to produce the deception in question; and if this be communicated to it gradually, the principal effect of *curvature* is observed to come on suddenly, but the *degree* of bending is independent of the velocity of the wheel, and each image appears, during the moment it is viewed, motionless. The number of spokes makes no difference in their degree of curvature. The deception is favoured by the diminution of the interval between the bars, by throwing a strong light on the wheel, and by every circumstance which tends to draw attention from the bars and fix it on the wheel. The number of curved images was found to depend on the ratio of the angles subtended at the eye by the intervals between the bars, and those between the extremities of the spokes; being greater as this ratio was less. If the bars were inclined to the horizon, the phenomena were those of a wheel revolving with a less velocity in a direction perpendicular to their length. Finally, the combination of a progressive with a rotatory motion (whether produced by a real motion of the wheel, the bars, or the spectator,) was found to be essential to the production of the phenomena.

From all these circumstances, the author is led to refer the explanation of the deception in question, to the principle that an impression made by a pencil of rays on the retina, if sufficiently vivid, will remain for a certain time after the cause has ceased. He then explains at length the application of this principle to the case in question, and shows that the apparent form of each spoke will be a curve formed by the continual intersection of the revolving and advancing radius, with the immoveable interval between the bars, referred, not to the plane in which the bars lie, for then it would be a straight line, but by an effort of attention, to the plane of the wheel. The general form of these curves he refers to the class of Quadratrices, and the most remarkable among them is that first discovered by Dinostrates, and known by his name. By varying a certain parameter in the equation of these curves, other forms, having infinite branches and points of contrary flexure, arise; and these

the author has succeeded in rendering visible, by making the wheel revolve on an axle of less than its own diameter; and the appearances being in this, as well as in the foregoing case, perfectly consonant to his theory, he considers the explanation given as quite satisfactory.

Dr. Roget concludes by suggesting the possibility of measuring the duration of the impression of light on the retina by observing the apparent velocity of the visible portion of the spokes.

On a new Photometer, with its application to determine the relative Intensities of Artificial Light, &c. By William Ritchie, *A.M., Rector of the Academy at Tain. Communicated by the President.* Read December 16, 1824. [*Phil. Trans.* 1825, *p.* 141.]

Mr. Ritchie, after a brief exposition of the theoretical views which led him to the construction of his photometer, lays down the following as the principles on which it depends:—

1. That radiant heat does not permeate glass.

2. That light is capable of combining with substances which stop it, and expanding them as heat does.

3. That the intensity of light is in the inverse ratio of the squares of the distance.

The photometer, which he then proceeds to describe, consists of two tin-plate cylinders, broad and shallow, each of which is closed at the one end with tin plate, and at the other with a disk of the thickest plate glass, both made air tight. Each of these cylinders or chambers contains in the middle a diaphragm of black paper, with its black side towards the glass, for the purpose (as he expresses it,) of absorbing the light which penetrates the glass, and instantly converting it into heat. The chambers are then fixed back to back at a little distance from each other, and connected by a bent tube in the form of the letter U, containing a small quantity of a coloured liquid.

This instrument is exposed with its glass faces opposite to two lights to be compared, and their equality is judged of by the liquid in the stem remaining stationary.

Its sensibility is described by the author as such, that a single candle placed 10, 20, or 30 feet from it visibly affects it; while a mass of heated iron affording twenty times the heat has no influence.

The author proposes his photometer as peculiarly well adapted to the measure of the quantity of light given out by gas lights. The solar light he describes as powerful enough to drive the liquid in the stem through 20 or 30 feet of tube. He states himself to have an instrument of this kind now making, with which he hopes to render sensible the effect of the moon's rays. Finally, he explains the difference between his instrument and that of Professor Leslie to consist in this,—that in the latter the difference of temperature between the two balls is the quantity measured; in the former the perfect equality of their temperatures is the essential condition.

The Description of a Floating Collimator. By *Captain* Henry Kater, *F.R.S.* Read January 13, 1825. [*Phil. Trans.* 1825, *p.* 147.]

The apparatus described in this paper (of which a drawing is now laid on the table) is intended to determine the situation of the line of collimation of a telescope attached to an astronomical circle, with respect to the zenith or horizon in some one position of the instrument; in other words, to determine the zero point of the divisions on the limb. This is at present usually performed by the use of the level or the plumb line, or by the reflection of an object from the surface of a fluid. The author describes the defects and inconveniencies of each of these methods. Those of the plumb line, when applied to small instruments (to the improvement of which he describes his attention to have been particularly directed,) are referrible chiefly to want of sufficient delicacy. Those of the level are referrible to a variety of causes not under the command of the observer; while observations, by reflection, the most perfect perhaps of any now practised. require an union of favourable circumstances rarely occurring. Add to these when levels or plumb lines are used, the necessity of reversing the instrument, and observing out of the meridian. And when observations are made by reflection, that of deferring the corresponding observation to the following night, which has proved so great an inconvenience at Greenwich, as to necessitate the erection of a second circle for the purpose of simultaneous observation.

The principles on which the floating collimator is constructed are two: the first is the property of a telescope employed by Mr. Gauss, and subsequently by Mr. Bessel, in virtue of which the cross wires of a telescope adjusted to distinct vision on the stars, may be distinctly seen by another telescope, also so adjusted, at whatever distance the telescopes may be placed, provided their axes coincide; the rays diverging from the cross wires of either telescope, emerging parallel from its object-glass, and being therefore refracted by that of the other telescope to its sidereal focus, as if they came from an infinite distance. The author here translates an account by Professor Bessel, of a method of using this principle to determine the horizontal or zenith point of a circle by the use of a level, employed to place the collimating or subsidiary telescope in a horizontal position, a method which though characterized by him as the best mode of using a level that has yet been devised, is still liable to the objections urged against levels in general.

The other principle which the author substitutes in the place of the level, is the invariability with respect to the plane of the horizon of a body of determinate figure and weight floating on the surface of a fluid. In former inquiries he had satisfied himself that a body floating on mercury might be so contrived as to have always, when at rest, the same inclination to the horizon. He had thus a floating support to which he could attach a telescope,—a support requiring no adjustment, offering the ready means of extreme accuracy, and precluding all fear of those errors which might arise from the use of a level.

The collimator in its perfect state consists of a piece of cast iron 8 inches long, 4 wide, and from $\frac{1}{4}$ to $\frac{1}{2}$ an inch thick, having two uprights in the form of Y's, to which the collimating telescope is firmly fastened. The support is then floated on mercury in a deal box, somewhat larger than the flat portion of the iron, and having its bottom just covered with mercury. The float is kept in its situation in the middle of the box, and prevented from moving horizontally by two smooth iron pins projecting from its sides, and moving freely in vertical polished grooves of metal let into the sides of the box. The whole of the telescope projects above the edges of the box, and a screen of black pasteboard with an aperture equal to that of its object-glass, is fixed to the end of the box to keep off false light. The instrument was placed on a table attached to the wall of the observatory, and directed (by looking through the telescope) to the wires of a fine achromatic furnished with a wire micrometer. The cross wires of the collimator were then illuminated by a small lantern placed behind its eye-glass with oiled paper interposed.

The object of the author in this arrangement being to ascertain the limits of variability, in the position assumed by the collimator, it was deranged purposely in a variety of ways, by removing and replacing the float, or carrying the whole instrument from its place, and every method he could think of used that could fairly introduce error. His preliminary trials were made with a wooden float; but this was soon laid aside after ascertaining that the greatest single error committed in using it, did not exceed $2''\cdot58$ in the position of the horizontal point. Other floats were then tried, and it was found that the increase of their length and browning their surfaces with nitric acid produced material advantages. In 151 single results thus experimentally obtained, 28 only were found to give errors in the determination of the horizontal point exceeding $1''$, and only two amounting to $2''$. But if the means of every successive 5 be taken, and the experiments with the wooden float rejected, the greatest error did not exceed $0''\cdot4$, and even here the influence of a constant source of error depending on the support of the micrometer employed was apparent.

The author then describes at length the mode of using the collimator and of observing with it. The instrument hitherto described may be called the horizontal collimator, but he then proceeds to describe a vertical collimator, in which the telescope is fixed perpendicularly to the float and placed immediately under the axis of the circle. By this arrangement the necessity of transporting it from one side of the observatory to the other is avoided, the reverse observation being made by merely turning the float half round in azimuth.

It is not necessary that the telescope of the collimator should have a tube, nor does the author appear to regard its length as of any importance, it being merely the direction of its axis which is the subject of examination; and the accuracy of this examination will depend on the length and power of the telescope of the circle to be collimated.

The adjustment of the cross wires in the exact sidereal focus of its object-glass is, however, a point of the highest importance.

The author next points out an important advantage which this instrument presents, viz. that of enabling the observer, by varying the inclination of his float, to detect erroneous divisions of his circle by bringing different parts of its arc into use; after which he proceeds to describe an application of his floating collimator, as a permanent verification of the verticality of a zenith tube, and considers that by its use the error, if any, in the zenith distance of a star, will be ultimately referred to inaccurate bisection of the star, or imperfections in the micrometer screws.

Notice on the Iguanodon, a newly discovered Fossil Reptile, from the Sandstone of Tilgate Forest, in Sussex. By Gideon Mantell, *F.L.S. and M.G.S. Fellow of the College of Surgeons, &c. In a Letter to* Davies Gilbert, *Esq. M.P. V.P.R.S. &c. &c. &c. Communicated by* D. Gilbert, *Esq.* Read February 10, 1825. [*Phil. Trans.* 1825, *p.* 179.]

The bones of the fossil herbivorous reptile described in this paper were discovered in the sandstone of Tilgate Forest in Sussex, which is a portion of the iron-sand formation, and forms a chain of hills stretching in a W.N.W. direction from Hastings to Horsham. In this sandstone the bones and teeth in question are accompanied with those of saurian animals, turtles, birds, fishes, shells, and vegetables, among which may be satisfactorily traced the remains of a gigantic species of Crocodile, of the Megalosaurus, and of the Plesiosaurus.

The teeth of the three last-mentioned animals are readily recognised and identified; but in the summer of 1822, others were discovered in the same strata, which, though evidently referrible to some herbivorous reptile, possessed peculiar and striking characters. Anxious to ascertain the opinions of naturalists respecting these, the author submitted them to the inspection of the most eminent, and among the rest to Baron Cuvier, who, while acknowledging that such teeth were previously unknown to him, agreed in the conclusion of their belonging to some herbivorous reptile of gigantic size, and recommended every research to be made for more connected portions of the skeleton.

Confirmed in his opinion by these remarks, the author renewed his researches with increased assiduity; and though no connected portions of the skeleton have hitherto rewarded his pains, some of the specimens were discovered in so perfect a state as to allow of a comparison with the teeth of recent lacertæ in the Museum of the Royal College of Surgeons; and the result of this comparison was, that in an Iguana there deposited, teeth were discovered possessing the form and structure of the fossil specimens.

Drawings both of the recent and fossil teeth accompany this paper, and were exhibited to the Society. They show a striking corre-

pondence in the serrated form of the edges of the teeth, in the ridges on their vertical surfaces, and particularly in the manner in which the new teeth are formed in lateral cavities at the base of the fangs of the old ones.

From the nature of the fossils with which these teeth are associated, the author concludes the Iguanodon to have been, if amphibious yet not marine, but an inhabitant of rivers and fresh-water lakes. Judging from the proportions of the recent iguana, he concludes that some of the fossil teeth figured in his paper must have belonged to an individual upwards of 60 feet long.

The author then considers the vertebræ, which differ materially from those of the recent iguana, crocodile, &c., and resemble rather those of the fossil crocodiles of Havre and Honfleur, being depressed at both extremities; but, as among recent lacertæ there are examples of the same structure in a higher degree, and the fossils in question are clearly of the saurian type, he does not regard the discrepancy as sufficiently important to invalidate the conclusions attempted to be established in this paper.

An experimental enquiry into the Nature of the radiant heating effects from terrestrial sources. By Baden Powell, *M.A. F.R.S. of Oriel College, Oxford.* Read February 17, 1825. [*Phil. Trans.* 1825, *p.* 187.]

In this paper the author first states the opinion of various preceding experimenters on the subject of the heat evolved from non-luminous sources, and from bodies in various degrees of luminosity, and observes that all the facts may be accounted for, by supposing two distinct heating influences, one associated in some very close way with the rays of light, and carried as it were by them through a glass screen without heating it, the other being merely simple radiant heat affected by the screen, exactly as the radiant heat from a non-luminous body.

In order to examine the truth of this explanation, he observes further, that it is not sufficient to observe the effects produced by the intervention of the screen alone, we must combine this with an inquiry into the relations to surfaces of the portions of the heat stopped and transmitted; that is to say, we must endeavour to discover whether the portions differ in any other respect than merely in transmissibility.

To this end the author institutes a set of experiments, whose general principle he states to be, "taking different luminous hot bodies, to expose to their influence two thermometers presenting, one a smooth black surface, the other an absorptive white one: thus obtaining the ratio of their total direct effects on the two, we may compare it with the ratio similarly observed, when a transparent screen is interposed."

After noticing some causes of fallacy necessary to be guarded

against, he proceeds to describe his apparatus, which consisted of two thermometers, A and B, of large and nearly equal bulbs graduated to quarters of centigrade degrees; one whitened with a wash of chalk and water, the other blackened with Indian ink. In other experiments a differential thermometer was used, one ball being only exposed to the heating influence under various coatings, and the other carefully screened, so as to reduce the effect as much as possible to that on a mere air thermometer.

A variety of experiments on the heating effects of various flames, incandescent metals, &c., on these thermometers so prepared, and both screened and unscreened with glass plates, are then detailed; and the author then draws his general conclusions, which may be thus stated.

1. That the heat radiated from all luminous hot bodies is divided or analysed by a glass screen into two portions, one of which is stopped by the screen, and employed in increasing its temperature, and the other passes through it without raising its temperature.

2. That besides this difference in the nature of the two portions of the total radiation, they differ in their capabilities of being absorbed by the surfaces of bodies. That portion which passes freely through glass being absorbed much more readily by blackened surfaces, while the other, or non-transmissible portion, is nearly equally well absorbed by black and white surfaces. The texture of surfaces, rather than their colour, he supposes to exercise the chief influence in determining the absorption of this latter portion, though this last-mentioned opinion is, perhaps, rather adopted in conformity with the language of others, than in consequence of any experiments detailed in the present paper.

On the Anatomy of the Mole-cricket. By J. Kidd, *M.D. and F.R.S. Reg. Prof. of Medicine in the University of Oxford.* Read February 3 and February 10, 1825. [*Phil. Trans.* 1825, *p.* 203.]

The insect described in this paper is common in certain peat bogs a few miles west of Oxford, and is found within 18 inches of the surface. Like the mole, its limbs are particularly calculated for burrowing; and to prevent the necessity of its excavating a passage large enough to admit of its turning round, it has the power of moving as easily in a retrograde as in a progressive direction. Its colour closely resembles that of the mould in which it lives; and in common with many other insects, it has the power of assuming a lifeless appearance when suddenly disturbed. Having kept some of them in glass vessels for several weeks, the author remarked that they preferred the potatoe to other vegetable food, but that they attacked raw meat with especial greediness, and upon emergency attacked each other, in which case the victor soon devoured the fleshy and soft parts of the vanquished. But although they are very voracious, they are equally remarkable for their power of abstaining from food, and have been

kept alive for nine or ten months in garden mould without the possibility of obtaining any other nourishment than such as it might contain.

Having noticed the general habitudes and characters of the insect, Dr. Kidd proceeds to describe its separate parts, and enumerates the peculiarities of the head, thorax, and abdomen. The digestive organs, he observes, more closely resemble those of a graminivorous bird than of any other animal; the œsophagus terminating in a large oval crop, communicating by a muscular tube with the gizzard, which is nearly spherical, and has a thick external muscular coat lined by a glandular membrane, the inner surface of which is divided longitudinally into six equal parts, each furnished with three series of serrated teeth of the hardness of tortoiseshell, and amounting in all to 270.

In his description of the organs of respiration of the Gryllotalpa, D. Kidd states, that ten stigmata are distinctly visible on each side of the body. The first of these, situated near the lower part of the posterior ridge of the thorax, is not like the others a mere dot or point, but an elongated fissure, apparently connected with all the tracheæ both of the thorax and head. To demonstrate the distribution of the tracheæ, the insect was dried in an exhausted receiver, containing muriate of lime, a method applicable to many delicate anatomical preparations; they penetrate every part of the body, and are possibly, in the author's opinion, the instruments of sanguineous circulation in insects, absorbing the blood in the first instance from the internal surface of the alimentary canal, and thence conveying it over the body. No difficulty, he apprehends, attaches to the supposition that such an absorption may take place, seeing that innumerable minute ramifications of the tracheæ penetrate the intestinal canal to every part. If, he continues, it should be urged that the tracheæ are not found charged with blood after the death of the animal, it may be answered, that the arteries are found empty after death in the higher orders of animals. He adds, that he has seen some of the ramifications of those tracheæ which are connected with the cæca, distended with a fluid of the same colour as that found in those organs; and though he has only witnessed this in two instances, yet such a fact even singly taken, must be allowed to be of considerable importance.

The author then adverts to the objections which may be urged against the hypothesis of the transudation of chyle through the coats of the intestines, trusting that his opinion of a sanguineous circulation in insects will not be hastily rejected; and concludes his paper with a description of the nerves and of the sexual organs of the insect; and with some remarks upon the organ of sound, which he considers as produced by the wings, and in no way connected with a peculiar tense membrane, situated between the fourth and fifth stigma on each side of the abdomen.

Further Observations on Planariæ. *By* J. R. Johnson, *M.D. F.R.S.* Read March 10, 1825. [*Phil. Trans.* 1825, *p.* 247.]

In an account of some species of Planariæ, published by Mr. Dalzell of Edinburgh, that gentleman observed, that in one of these insects which he had intentionally wounded a little below the head, an unnatural prominence soon appeared at the wounded part, which in about four weeks assumed the characters of a new head, and was soon after very distinctly recognised as such.

Anxious to verify so singular a result, Dr. Johnson took 100 active *Planariæ cornutæ*, and made in each an incision on the side of the body. In one instance only he obtained the desired result, in most of them the wounds healed. In some, præternatural excrescences took place, and others separated at the place of incision to become two animals. One only acquired a double head. It appears, therefore, to be so unusual an occurrence as to deserve the attention of the Royal Society.

Dr. Johnson proceeds to some additional observations respecting the propagation of certain species of this curious tribe of insects. In regard to the *Planariæ cornutæ*, he found that they were more rapidly reproductive by the detachment of fragments when kept singly than when several are preserved in the same vessel, owing, he says, to the necessity then existing of continuing the species; hence he also infers, that the regenerative process is voluntary. The Planariæ kept together, ultimately threw off as many reproductive portions as the others, and these he thinks may probably amount to about 20 in eight months for each insect. The smallest visible portion detached from the tail becomes a perfect Planaria, but in this case the animal is so small as to suggest the probability of the parent animal being viviparous.—Dr. Johnson concludes this paper with some observations on the *Planaria nigra*, the details of which are illustrated by an annexed drawing. Like the species formerly described, it is furnished with an abdominal proboscis, by which it takes its food; it is oviparous, each producing from 2 to 6 young; it does not spontaneously divide into regenerative portions like the *Planaria cornuta*, but has an equal power of repairing mutilated parts.

On the Influence of Nerves and Ganglions in producing Animal Heat. By Sir Everard Home, *Bart. V.P.R.S. presented by the Society for the Improvement of Animal Chemistry.* Read March 17, 1825. [*Phil. Trans.* 1825, *p.* 257.]

Sir Everard begins this paper by adducing several instances of the existence of brain and nerves in animals, which however have no power of generating heat; this is the case with the Oyster, the Snail, and the Water-muscle. In the Leech, the Earthworm, and the insect tribe generally, the nervous filaments are united at intervals by ganglions; and where these exist, the temperature exceeds that of the atmosphere when below 56°, though in very different degrees, the

xcess in the leech being only 1°; while in a hive of bees it is 26°. hese circumstances induced the author to inquire whether any parts f animals possessed of an unusual temperature were devoid of nerves: he heat of the deer's horn while inclosed in its velvet, was found in he month of June, when only one foot long, to be 96°, and in July he top of the antler was 99°; the power therefore of generating heat as here so evident, independent of any direct influence of the brain r heart, that it was only necessary to ascertain whether nerves ac-ompanied the blood-vessels, and they were found to do so very nu-nerously. To ascertain how far animal heat was under the control f the ganglionic nerves, the trunks supplying the velvet of one horn f the deer were divided, while those of the other were left entire; nd the result was, that on the first day the temperature fell 12° hort of that of the latter; on the second day 26°; on the third day 17°; on the fourth day 8°; and on the fifth 2°. Forty-eight hours fter the division of the nerves, the temperature of the horn fell to ithin 3° of that of the atmosphere, but the animal having bruised he horn, the diary was discontinued on the sixth day, and it was hen hotter than that of which the nerves were entire; and although he nervous trunk had not reunited, it was evident that some other onnection had been formed between the nerves of the horn and head. The author next adverts to the abundant connexion of the placental erves with ganglia, as described by Mr. Hawkins on a former oc-asion. This led him to suspect that the uterus might under particular ircumstances of excited action, possess peculiar powers of generating eat; and he was informed upon inquiry of practitioners in midwifery, hat they sometimes found in turning children, the heat almost as reat as the hand could endure. Sir Everard then details the results f some experiments in relation to this subject, which were furnished by Dr. Granville, showing that in certain cases of difficult labour, the temperature of the uterus rises as high during the violence of the pains as 120°; and to prove that mere muscular action is not the ause of this production of heat, he observes that the temperature f the heart of a dog in full action is only 101°, and that in certain ases of apoplexy the body becomes alternately hot and cold, the pulse undergoing no variation in its frequency.

Sir Everard concludes this communication with some remarks upon the relation between the proportion of ganglionic nerves and that of temperature above the surrounding medium in certain fishes, and ob-serves that they always appear to bear a direct proportion to each other. He also notices the enormous supply of nerves sent to the lectric organs in the Gymnotus and the Torpedo, which, however, re productive of no remarkable increase of temperature, being en-tirely free from, and unconnected with, any ganglionic arrangement. He also suggests the probability of the ganglionic nerves being those which are principally affected in all diseases attended by a consider-able elevation of heat beyond the natural standard.

An Essay on Egyptian Mummies; with Observations on the Art of Embalming among the ancient Egyptians. By A. B. Granville, *M.D. F.R.S. F.L.S. F.G.S. M.R.I. one of His Royal Highness the Duke of Clarence's Physicians in Ordinary, &c. &c.* Read April 14, 1825. [*Phil. Trans.* 1825, *p.* 269.]

The mummy described in this paper was purchased at Gournou; it was in a single case of the usual form, and covered with cere cloth bandages, very neatly and dexterously applied, and among which both cotton and linen were recognised. These, to the amount of 28 pounds avoirdupois in weight, having been removed, the body was discovered to be that of a female. The abdominal integuments were remarkably wrinkled, and the whole surface of a dark brown colour and dry, but in many places soft to the touch, and with the exception of a few parts, entirely deprived of cuticle. The height of the mummy from the vertex of the head to the inferior surface of the calcaneum was 5 feet $\frac{7}{10}$ inch, and the principal dimensions of the several parts correspond with those which are usually considered as giving rise to the utmost perfection of female form in the European race, neither was any trait of Ethiopian character discernible in the form of the cranium; all which, observes Dr. Granville, supports Cuvier's opinion respecting the Caucasian origin of the Egyptians.

The author then proceeds to a brief summary of the present state of our information respecting Egyptian mummies, attributing its scantiness and imperfection to the rarity of perfect specimens, nearly all the mummies hitherto described, presenting little else than imperfect skeletons enveloped in bandages, sometimes covered by the dry skin.

In proceeding to examine the present specimen, the integuments and muscles of the abdomen were first removed, and the contents of that cavity carefully inspected. They consisted of a portion of the stomach adhering to the diaphragm, the spleen attached to the suprarenal capsule of the left kidney, and the left kidney itself, with the ureter descending into the bladder, which with the uterus and its appendages were observed *in situ*, the latter exhibiting marks of disease. Fragments only of the intestinal tube were discoverable; and there were a few lumps of resin, of a compound of clay and bitumen, and a few pieces of myrrh. The right kidney, the liver, and minor glands were missing, but the gall-bladder was detected among the loose fragments of membranes and other soft parts, together with remains of its own ducts. The soft parts of the pelvis were then particularly examined, and the perfect condition of the muscles, membranes, and ligaments particularly noticed. The cavity of the thorax was next examined, by detaching the diaphragm, to which part of the pericardium adhered; and the heart, in a very contracted state, was afterwards found suspended by its vessels, and attached to the lungs, which adhered to the ribs.

Upon the examination of the cranium, it was evident that the brain had been removed through the nostrils, from the lacerated condition

of the inner nasal bones, the eyes appear not to have been disturbed, the tongue was entire, and the teeth white and perfect.

Dr. Granville next proceeds to draw some conclusions as to the age at which this mummied female died, and respecting the disease which destroyed her. The bones of the ileum exhibit that peculiar thinness of their osseous plates which show the individual to have exceeded her fortieth year, and to have borne children; and as there are no characters of age or decrepitude about the skeleton, the author considers her to have been about fifty. The ovarium and broad ligament of the right side were enveloped in a mass of diseased structure, while the Fallopian tube of the same side was sound; but the uterus itself was larger than natural, and the remains of a sac were found connected with the left ovarium; all which, connected with the appearance of the abdominal integuments, leave no doubt of ovarian dropsy having been the disease under which the individual suffered.

The author concludes this communication with some observations respecting the method of embalming generally, and the nature of the substances employed in the process, from the details of which he draws the following conclusions:—

The abdominal viscera were more or less perfectly extracted, either through an incision on one side of the abdomen, or, as in the present mummy, through the anus. The thoracic cavity was not disturbed. The contents of the cranium were removed sometimes through the nostrils, and in others through one of the orbits. The body was then probably covered with quick-lime, to facilitate the removal of the cuticle, the scalp and nails being, however, left untouched; after which, it was immersed in a melted mixture of wax, resin, and bitumen, until thoroughly penetrated; and ultimately subjected to a tanning liquor, probably made with the saline water of the neighbouring natron lakes. The bandages were applied with the occasional interposition of melted resin, or wax and resin, the lumps of resin, myrrh, &c., having been previously placed in the abdomen.

On the temporary Magnetic Effect induced in Iron Bodies by Rotation. In a Letter to J. F. W. Herschel, *Esq. Sec. R.S. by* Peter Barlow, *F.R.S. Communicated April 14th,* 1825. Read May 5, 1825. [*Phil. Trans.* 1825, *p.* 317.]

The author's attention having been recalled to the consideration of the effects of rotation in altering the magnetic influence of iron, in the course of speculations on the cause of the rotation of the earth's magnetic poles; and knowing, at the same time, that Mr. Christie had found a permanent change in the magnetic state of an iron plate, by mere change of position on its axis, it seemed to him highly probable that this change, due only to a simple inversion, would be increased by rapid rotation. On trial, however, it was found that the effect produced was merely temporary. The experiments at first were made with a 13-inch mortar shell, fixed to the mandrel of a

powerful turning lathe, worked by a steam-engine in the Royal Arsenal at Woolwich. This being made to revolve at the rate of 640 turns per minute, the needle was deflected out several degrees, and there remained stationary during the motion of the ball, but returned immediately to its original position on ceasing the rotation. On inverting the motion of the shell, an equal and contrary deflection took place.

As the law of the phenomena was not evident with this disposition of the apparatus, and the shell was found too heavy for perfect safety, a Shrapnel shell of eight inches diameter was mounted in a proper apparatus (described in the paper), and a number of experiments made; the law of which, however, still seemed anomalous, till the idea occurred of neutralizing the earth's action on the needle, when the anomalies disappeared, and the general law of the effect was placed in evidence. The needle being made a tangent to the ball, if the motion of the ball was made towards the needle (whatever was the direction of the axis of rotation), the north end of the latter was attracted, and if the contrary way, repelled. In the two extremities of the axis there was found no effect, while in two opposite points, at right angles to the axis, the effect was a maximum, and the direction of the needle was to the centre of the ball.

The author then proceeds to show how all the results, which before appeared anomalous, agree with this general view, and closes his communication with some theoretical views of their general bearing on the subject of the earth's magnetism, which he thinks there are strong reasons for believing to be of the *induced* kind; and although it appears to him doubtful whether the anomalies observed in the variation of the needle on the earth's surface, can ultimately be referred to this cause, yet, he observes, that one condition essential to the production of these phenomena holds good in the case of the earth, viz. the non-coincidence of its polarized axis with that of its diurnal rotation.

Further Researches on the Preservation of Metals by Electro-chemical Means. By Sir Humphry Davy, *Bart. P.R.S.* Read June 9, 1825. [*Phil. Trans.* 1825, *p.* 328.]

After adverting to the general details respecting the protection of the copper sheathing of ships, contained in his former papers, the President proceeds, in the present communication, to consider the circumstances under which various substances are deposited upon the protecting copper, and their general influence upon its wear, more especially in regard to ships in motion. For this purpose, he availed himself of the use of a steam boat, employed on an expedition to ascertain some points of longitude in the North Seas, and his inquiries lead to the inference that motion does not affect the nature of the limits and quantity of the protecting metal; and that, independently of the chemical, there is likewise a mechanical wear of the copper in sailing.

In examining the results of some of the experiments upon the ffects of single masses of protecting metal on the sheeting, the uthor observed, that in some cases the corrosion seemed to increase rith the distance from the protector. It became, therefore, necesary to investigate this circumstance, and to ascertain the extent of he diminution of electrical action in instances of imperfect or irreular conducting surfaces. Sir Humphry details several experiments n illustration of this inquiry, which prove that any diminution of rotecting effect at a distance does not depend upon the nature of he metallic, but of the imperfect or fluid conductor. His experiaents upon perfect and imperfect conductors led him to another inuiry, important in its practical relations, respecting the nature of he contact between the copper and the preserving metal. He found he protecting action prevented by the thinnest stratum of air, or the nest leaf of talc or dry paper; but the ordinary coating of rust, or thin piece of moistened paper, did not impair it.

After some experimental details respecting the electro-chemical owers of metals in solutions excluded from air, Sir Humphry conludes his paper with practical inferences and theoretical elucidations rising out of its general details. Finding that in certain cases of mperfect connexion, the influence of the protector was weakened y distance, the author proposed that when ships with old sheathing rere to be protected, a greater proportion of iron should be used, nd if possible more distributed. The advantage of this plan was trikingly shown in the Semerang, which had been coppered in India in the year 1821, and came into dock, in the spring of 1824, overed with rust, weeds, and zoophytes; she was protected by four nasses of iron, equal in surface to about one 80th of the copper, two f which were near the stern, and two on the bows. She made a oyage to Nova Scotia, and returned in January 1825; not, as was alsely reported, covered with weeds and barnacles, but remarkably lean and in good condition. After citing other instances of the erfect efficiency of the protectors, and adverting to the relative proortion which, in different circumstances, they ought to bear to the heathing of the vessel, and to the most advantageous methods of pplying them, the author concludes by observing upon the importnce of selecting perfectly pure copper for the sheathing; of applyng it smoothly and equably; and of using for its attachment nails f pure copper, and not of mixed metal.

On the Magnetism of Iron arising from its Rotation. By Samuel Hunter Christie, *Esq. M.A. of Trinity College, Cambridge; Fellow of the Cambridge Philosophical Society; of the Royal Military Academy. Communicated April* 20, 1825, *by* J. F. W. Herschel, *Esq. Sec. R.S.* Read May 12, 1825. [*Phil. Trans.* 1825, *p.* 347.]

The effects observed and described in this paper, although minute n themselves, appear, in the author's opinion, to point out a species of magnetic action not hitherto described. It had long been well

known that striking, twisting, or filing iron in different directions with regard to the magnetic axis, materially influenced its polarity; but it does not appear to have been remarked, that the simple rotation of iron in different directions has any such influence. This, however, the author has ascertained to be the case; and that the laws which govern this peculiar action are so regular, that there can remain no doubt of a corresponding regularity in their causes.

The attention of the author was first drawn to these phenomena by some apparent anomalies in the magnetic action of an iron plate on the compass, observed in the course of a different investigation. In order to avoid or allow for the disturbing influence of partial magnetism in the iron, it became necessary to attend minutely to the position of certain points in its circumference, which corresponded to the maxima and minima of this magnetism. It was then found that these points were not constant, but shifted their position as the plate was made to revolve in its own plane; or, in other words, that a plate which, in a given position, produced a certain deviation in a compass, no longer produced the same deviation after making an exact revolution in its own plane, although brought to rest, and every part of the apparatus restored precisely to its former place. It appeared from this that the revolution of the plate in its own plane had an influence on its power of deviating the needle independent of the partial magnetism of particular points in it; and the justice of this idea was proved by giving it a rotation in an opposite direction, when the effect on the directive power was also reversed.

The change produced by rotation in the directive power of the plate was found to be a maximum when its plane was parallel to the line of dip, or the magnetic axis, and at the same time as little inclined to the horizon as this condition would allow; but when the plane of the plate was parallel to the horizon, the effect was diminished in the ratio of 5 to 1; and when perpendicular to the horizon, and coincident with the magnetic meridian, was altogether destroyed.

The author having satisfied himself of the reality and constancy of this effect in different plates, and of the necessity of referring it to a peculiar agency of the earth's magnetic power on the molecules of the plate, proceeded to ascertain the laws, and measure the quantities of the *deviation* due to rotation (so he terms it) in various positions, and details a great number of experiments, with their numerical results arranged in the form of tables. From these he deduces the following general law; viz. That the deviation due to *rotation* in a *dipping-needle* "will always be such that the sides of the equator of such dipping-needle will deviate in a direction contrary to the direction in which the edges of the plate move; that edge, and the plate nearest to either edge of the equator, producing the greatest effect."

The results of this law, it may be here observed, are in many cases coincident with those of the following. Conceive the dipping-needle orthographically projected on the plate; then will the *deviation due*

to rotation of the projected needle *take place in a direction opposite to that of the rotation itself.*

The author then proceeds to a theoretical investigation of the effect of a plate of soft iron, having within it two poles developed in given positions, and acting (in addition to the usual magnetic action of soft iron,) on a needle of infinitely small dimensions in the plane of the plate. He refers the whole ordinary action of the iron to its centre, and supposes that this is *attractive* on both poles of the needle; but the extraordinary action, or that of the newly developed poles, he supposes to reside in them, and to be attractive or repulsive according as they act on the poles of the needle of the same or opposite names with themselves. On this hypothesis, assuming symbols for the coordinates of the plate's centre, the distances separating the newly developed poles in the plate, and the angle which the line joining them makes with the direction of the needle, &c. &c. deduce (from the known laws of magnetism) formulæ expressing the horizontal deviations of the needle: 1st, on the supposition of a rotation in one direction; 2ndly, on that of a rotation in the opposite; and 3rdly, in that of no rotation at all. From these, by comparing them with a few of the observations, he deduces numerical values for the constants of the formulæ, and then employs them to compute the deviations due to the rotation in all the rest. He regards the discrepancy between the calculated and observed results as in few cases larger than what he considers may be fairly attributed to error of observation, and that the theory above stated is at the least a general representation of what passes in fact; admitting, however, that it does not give the exact position of the point where the deviation due to the rotation vanishes, and suggesting partial magnetism in the iron plate used, as one mode of accounting for the difference. At all events, by an examination of the case on the ordinary supposition of induced magnetism in the iron, he shows that a greater coincidence between theory and fact would not result from that hypothesis than from the one here employed.

He then proceeds to inquire into the degree of permanence of the polarity thus produced in iron by rotation; from which inquiry it appears that (at least during twelve hours after the plate was brought to rest,) the influence of a single rotation had scarcely suffered any diminution. It appears also that the effect is so far from depending on the rapidity of the motion, that the plate can scarcely be made to revolve so slowly that the whole effect shall not be produced.

Lastly, by a slight change in the formulæ, the results of computation can be made to agree with observation, to a degree of exactness as near as can be wished. This change consists in the omission of certain terms introduced by the theory, and the author regards it as very possible so to modify the theory as to get rid of them.

The author closes this communication with an Appendix, comparing the magnetic effects produced by slow and rapid rotation. The result of the comparison is, that the forces exerted on the needle

during rapid rotation, are always in the same direction as those derived from the slowest rotation, and which continue to act after the rotation has ceased, but are greater in intensity; and that the former effects are such as might have been looked for from a knowledge of the latter.

Some Account of the Transit Instrument made by Mr. Dollond, *and lately put up at the Cambridge Observatory. Communicated April* 13, 1825. *By* Robert Woodhouse, *A.M. F.R.S.* Read May 19, 1825. [*Phil. Trans.* 1825, *p.* 418.]

The author in this paper first describes the operations by which the new transit instrument at the Observatory of Cambridge was approximatively placed, so as to allow of a meridian mark being erected on the distant steeple of Granchester church. He then enters into a more full consideration of the different methods proposed and employed by astronomers for executing the more delicate adjustments of the transit in general; he shows how the errors of collimation, level, azimuth, and the clock, may all be detected, and their values determined, by the resolution of certain equations of the first degree, constructed from observations of any three or more stars; but this method, though exact in theory, he reprobates in practice, and prefers making each adjustment separately and by the ordinary mechanical trials, as shorter, more effectual, and less troublesome. Mr. Woodhouse then describes a remarkable phenomenon presented to him by the transit in the course of his observations. He found that the line of collimation of the instrument deviated occasionally to the east or west of the centre of the meridian mark, without any apparent reason. At length, however, it was found that this was caused by the approach of the assistant's body to the lateral braces, placed for the purpose of steadying the instrument in an invariable position at right angles to its axis. The expansion of the brace nearest to him was found to thrust the axis of the telescope aside; and on the removal of the assistant, the equilibrium of temperature restoring itself, the deviation gradually disappeared. That this was the true cause, appeared by wrapping hot cloths round the alternate braces, by which the same effect was produced in an increased degree. Warned by these observations, Mr. Woodhouse ordered a proper apparatus to be provided, to defend the braces from the sun's rays, during the meridian passage of that luminary.

On the fossil Elk of Ireland. By Thomas Weaver, *Esq. Member of the Royal Irish Academy, of the Royal Dublin Society, and of the Wernerian and Geological Societies.* Read May 19, 1825. [*Phil. Trans.* 1825, *p.* 429.]

Mr. Weaver's principal object in this paper is to prove that the remains of the gigantic elk, which have been found in various parts of Ireland, are not of antediluvian origin, but that the animal lived

and flourished in the countries in which its remains are now found, at a period of time which, in the history of the earth, may be considered only as modern; and that the extinction of the species is attributable rather to the continued persecution it endured from its enemies, accelerated by incidental local causes, than to any general catastrophe that overwhelmed the surface of the globe.

The spot examined by the author containing these remains, is near the village of Dundrum, in Down: it appears formerly to have been a lake, and is now covered with peat lying upon a bed of marl. The bones are invariably found between these two substances, and from the examination of the shells contained in the latter, it appears that they are exclusively fresh-water species.

The peat bog of Rathcannon, in the county of Limerick, has also furnished abundance of the same bones, similarly situated. These were examined by the Rev. Mr. Maunsell before they were displaced. Some of them showed marks of disease and fractures, and in one case the rib was singularly perforated, as if by a sharp instrument. Marrow, having the appearance of fresh suet, was found in the cavity of one shank-bone, and they appeared generally to contain all the principles found in fresh bones.

These and some other concurrent circumstances seem, says the author, to remove all idea of the remains of the Irish elk being of any other than comparatively recent origin; and in seeking for a cause of the nearly constant distribution of these remains in Ireland in swampy spots, he conjectures that the animal may have often sought the waters and the marshy land as a place of refuge from its enemies, and thus not unfrequently found a grave where it looked for protection.

Microscopical Observations on the Materials of the Brain, and of the Ova of Animals, to show the analogy that exists between them. By Sir Everard Home, *Bart. V.P.R.S. Read at the Society for promoting Animal Chemistry, April* 12, 1825. Read at the Royal Society June 3, 1822. [*Phil. Trans.* 1825, *p.* 436.]

The author first details the results of some experiments made with a view to ascertain whether frogs, that had been completely frozen, could, under any circumstances, be restored to life, which he found never to be the case when the brain had been entirely congealed, the substance of which, after such process, never regains its former appearance, but is dissolved into a watery fluid, mixed with some gelatinous matter. In the act of freezing, the human brain was found to suffer a similar decomposition; the molecule of a pullet's egg is also resolved during the process of freezing into materials corresponding with those of the brain, and the testicular secretion was found to be similarly constituted, and in no instance to contain animalcules, as Leuenhoek and other more recent authors have affirmed. Magnified drawings, executed by Mr. Bauer, of the various substances described in this paper, accompany the communication.

On new Compounds of Carbon and Hydrogen, and on certain other Products obtained during the Decomposition of Oil by Heat. By M. Faraday, *F.R.S. Cor. Mem. Royal Academy of Sciences of Paris, &c.* Read June 16, 1825. [*Phil. Trans.* 1825, *p.* 440.]

The experiments of which the results are detailed in this paper, were made principally on the fluid which is found to be deposited in considerable quantity when oil-gas is compressed. This fluid, as obtained at the works of the Portable Oil-gas Company, is colourless, of a specific gravity less than that of water; insoluble in water except in very minute quantities; soluble in alcohol, ether, oils, &c.; and combustible, burning with a dense flame. It is strikingly distinguished from the oil from which it originated, by not being acted upon to any extent by solutions of the alkalies.

Part of this fluid is very volatile, causing the appearance of ebullition at temperatures of 50° or 60°; other parts are more fixed, requiring even 250°, or above, for ebullition. By repeated distillations a series of products were obtained from the most to the least volatile, the most abundant being such as occurred from 170° to 200°. On subjecting these, after numerous rectifications, to a low temperature, it was found that some of them concreted into a crystalline mass, and ultimately a substance was obtained from them, principally by pressure at low temperatures, which upon examination proved to be a new compound of carbon and hydrogen. At common temperatures it appears as a colourless transparent liquid, of specific gravity 0·85, at 60°; having the general odour of oil-gas. Below 42° it is a solid body, forming dendritical transparent crystals, and contracting much during its congelation. At 0° it appears as a white or transparent substance, brittle, pulverulent, and of the hardness nearly of loaf-sugar. It evaporates entirely in the air: when raised to 186° it boils, furnishing a vapour, which has a specific gravity of 40, compared to hydrogen as 1. At a higher temperature the vapour is decomposed, depositing carbon. The substance is combustible, liberating charcoal if oxygen be not abundantly present. Potassium exerts no action upon it below 186°.

This substance was analysed by being passed over red-hot oxide of copper, and by detonation of its vapour with oxygen. The results obtained were, that it consists of 2 proportionals of carbon, and 1 of hydrogen = 13; and that in the state of vapour 6 proportionals of carbon and 3 of hydrogen are present to form 1 volume, which is consequently of the specific gravity of 39, hydrogen being 1. It is named in the paper *bicarburet of hydrogen*.

Experimenting with the most volatile portions of the liquid, a portion was obtained, which, though gaseous at common temperatures, condensed into a liquid at 0°. This was found to be very constant in composition and properties: it was very combustible: it had a specific gravity of 27 or 28 as a gas, as a liquid that of 0·627, being the lightest substance, not a gas or vapour, known. When analysed, it was found to consist of one proportional of carbon 6, and one of

hydrogen 1, as is the case with olefiant gas; but these are so combined and condensed as to occupy only one half the volume they do in that substance. A volume therefore of the gas contains four proportionals of carbon 24, and four of hydrogen, 4 = 28, which is its specific gravity.

Beside the remarkable difference thus established between this substance and olefiant gas, it is also distinguished by the action of chlorine, which forms with it a fluid body, having a sweet taste, and resembling hydrochloride of carbon, but from which a chloride of carbon cannot be obtained by the further action of chloride and light.

The other products from the original fluid do not present any characters so definite as the above substances; at the same time they appear to be very constant, boiling uniformly at one temperature. They cannot be separated by distillation into more and less volatile parts, so as to afford means of reducing their number to two or three particular bodies. They have the general properties of the original fluid, and with the other products, are all peculiarly acted upon by sulphuric acid, offering phenomena, in the investigation of which the author is at present engaged.

With reference to the presence of these substances in the state of vapour in oil- and coal-gas, the means of ascertaining it and the quantity are pointed out, in the peculiar action of sulphuric acid, causing their perfect condensation, and in the solvent powers over them possessed by fixed and volatile oils, &c.; the requisite precautions for their proper application being pointed out. Oil-gas was found to be saturated with many of these vapours: coal-gas also contained a portion of them.

The paper concludes with a short reference to the probable uses of the fluid, as originally obtained. If put into gas burning with a blue flame, it makes it produce a bright white flame; it is an excellent solvent of caoutchouc; it will answer all the purposes to which essential oils are applied as solvents; and having applied that portion of it which, though a liquid at common temperatures and under a pressure of two or three atmospheres, is a gas under any diminished pressure, as fuel to a lamp; the author has shown the possibility of such an application, if at any time such knowledge and command of the decomposition of oil or coal by heat should be obtained, as would enable us to furnish the substance in abundance.

Account of the Repetition of M. Arago's *Experiments on the Magnetism manifested by various Substances during the Act of Rotation. By* C. Babbage, *Esq. F.R.S. and* J. F. W. Herschel, *Esq. Sec. R.S.* Read June 16, 1825. [*Phil. Trans.* 1825, *p.* 467.]

The experiments of M. Arago having excited much interest, the authors of this communication were induced to erect an apparatus for their verification; and after a few trials they succeeded in causing

a compass to deviate from the magnetic meridian, by setting in rotation under it plates of copper, zinc, lead, &c.

To obtain more visible and regular effects, however, they found it necessary to reverse the experiment, by setting in rotation a powerful horse-shoe magnet, and suspending over it the various metals and other substances to be examined, which were found to follow with various degrees of readiness the motion of the magnet. The substances in which they succeeded in developing signs of magnetism were, copper, zinc, silver, tin, lead, antimony, mercury, gold, bismuth, and carbon, in that peculiar metalloidal state in which it is precipitated from carburetted hydrogen in gas-works. In the case of mercury the rigorous absence of iron was secured. In other bodies, such as sulphuric acid, resin, glass, and other non-conductors, or imperfect conductors of electricity, no positive evidence of magnetism was obtained.

The comparative intensities of action of thin bodies were next numerically determined by two different methods, viz. by observing the deviation of the compass over revolving plates of great size cast to one pattern, and by the times of rotation of a neutralized system of magnets suspended over them; and it is curious that the two methods, though they assigned the same order to the remaining bodies, uniformly gave opposite results in the cases of zinc and copper, placing them constantly above or below each other according to the mode of observation employed.

The authors next investigated the effect of solution of continuity on the various metals, in the course of which M. Arago's results of the diminution of effect, by division of the metallic plates used, were verified; and the further fact ascertained, that re-establishing the metallic contact with other metals, restores the force either wholly or in great measure, and that even when the metal used for soldering has in itself but a very feeble magnetic power, thus affording a power of magnifying weak degrees of magnetism.

The law of diminution of the force by increase of distance is next investigated. It appears to follow no constant progression according to a fixed power of the distance, but to vary between the square and the cube.

The remainder of this paper is devoted to reasoning on the facts detailed. The authors conceive that they may be all explained without any new hypothesis in magnetism, by supposing simply that time is requisite both for the development and loss of magnetism; and that different metals differ in respect not only of the time they require, but in the intensity of the force ultimately developable in them; and they apply this explanation not only to their own results, but to those obtained by Mr. Barlow in his paper on the rotation of iron.

On the Magnetism developed in Copper and other Substances during Rotation. In a Letter from Samuel Hunter Christie, *Esq. M.A. &c. to* J. F. W. Herschel, *Esq. Sec. R.S. Communicated by* J. F. W. Herschel, *Esq.* Read June 16, 1825. [*Phil. Trans.* 1825, *p.* 497.]

Mr. Christie in this communication gives an account of some experiments on the development of magnetism in copper by rotation. He corroborates by his own experience the results obtained by Mr. Babbage and Mr. Herschel, in which a disc of copper was set in rotation by the rotation of one or more magnets beneath it, both in the case when poles of the same name were immediately below the disc and when of a contrary name. The action appeared equally intense in both cases, and from this circumstance the author concludes the magnetism thus communicated to the copper to be extremely transient. The experiment was varied by combining the revolving magnets differently, and the results are stated.

The next experiments of Mr. Christie were directed to the determination of the law according to which the force diminishes as the distance between the disc and magnets increases. It seems to follow from these experiments, that when a thick copper plate is made to revolve under a small magnet, the force tending to deviate the needle is directly as the velocity, and inversely as the fourth power of the distance; but that when magnets of considerable size are made to revolve under these copper discs, the diminution follows more nearly the ratio of the inverse square of the distance, or between the square of the cube, though not in any constant ratio of an exact power.

The author then investigates the law of force when copper discs of different weights are set in rotation, which for small distances appear proportioned to the weights of the discs, but for greater ones appear to vary in some higher ratio.

On the annual Variations of some of the principal Fixed Stars. By J. Pond, *F.R.S. Astron. Royal.* Read June 16, 1825. [*Phil. Trans.* 1825, *p.* 510.]

This communication consists of a table stating the annual variations of 23 of the principal fixed stars, as deduced from Dr. Brinkley's observations, and those of the Astronomer Royal. On these Mr. Pond remarks, that out of 16 stars observed at Dublin, 13 either indicate a southern deviation, or at least are not inconsistent with it, and that of these 13, about half indicate a greater deviation than that assigned by Mr. Pond himself. The other half a less, while the three remaining stars deviate northwards.

Mr. Pond further remarks, that the examination of this table is calculated rather to increase than to diminish scepticism on the subject of the determination of such very small quantities by astronomical observations. He concludes by disclaiming all intention of placing

the subject in a controversial point of view, and by expressing a hope that the difficulty will in a very few years be satisfactorily cleared up.

On the Nature of the Function expressive of the Law of Human Mortality, and on a new Mode of determining the Value of Life Contingencies. In a Letter to Francis Baily, *Esq. F.R.S. &c. By* Benjamin Gompertz, *Esq. F.R.S.* Read June 16, 1825. [*Phil. Trans.* 1825, *p.* 513.]

This paper, which professes to be a continuation of former researches on the same subject printed in the Transactions of the Royal Society, is divided into two chapters. In the first the author considers the nature of the law of those numbers in tables of mortality, which express the amount of persons living at the end of ages in regular arithmetical progression. He remarks that for short intervals the law approaches nearly to a decreasing geometrical progression, and that this must be the case whatever be the strict expression for the law of mortality, provided the intervals do not exceed certain limits. But he further remarks, that this property will be found to belong to very extensive portions of tables of mortality, and instances Deparcieux's tables, where from the age of 25 to that of 45, the numbers living at the end of each year decrease very nearly in geometrical progression.

Considering however the whole extent of such a table, it will be found that the ratio of this geometrical progression is not the same in all parts of the table. But before he enters on this consideration, the author draws some consequences from the hypothesis of a geometrical progression being the strict law of nature after a certain age. One of these is the equality of value of all life annuities commencing after that age. Another is, that the want of instances in history of persons living to very enormous ages (waving those of the patriarchs,) is no proof that such may not be the law of nature, as he shows by calculation, that out of 3,000,000 persons of 92, not more than one should on this supposition reach 192. This leads him to some general considerations on the causes of death, after which he resumes the consideration of the general law of the tables.

To find this *à priori*, he supposes that a person's resistance to death decreases as his years increase, in such a manner, that at the end of equal infinitely small intervals of time, he loses equal infinitely small portions of his vital powers. He further supposes, that among any given number of persons of equal vital powers the probability of death is the same, but that among all, it is inversely proportional to the vitality. These postulata being assumed, he enters into an analytical investigation, the result of which is a representation of the law of life by such a function as is sometimes called a double exponential, that is, a geometric progression in which the ratio is itself variable in geometric progression.

He then proceeds to examine the coincidence of this law, with several tables of the best authority, such as those of Deparcieux,

Northampton, the Swedish and Carlisle tables, and the supposed experience of the Equitable Assurance Office. The results of these comparisons are stated in a tabular form, and are very favourable to the law supposed.

In the second chapter, the author, after briefly explaining by algebraical reasoning, the mode of applying these principles to calculations of annuities, proposes general tables for facilitating this application in practice. These tables (which occupy 28 folio pages, and represent the logarithm of the present values of annuities for every value of a certain argument,) are actually calculated, and annexed to the communication, forming the principal part of the second chapter.

Observations of the apparent distances and positions of 458 *Double and Triple Stars, made in the years* 1823, 1824, *and* 1825; *together with a re-examination of* 36 *Stars of the same description, the distances and positions of which were communicated in a former Memoir. By* James South, *Esq. F.R.S.* Read November 17, 1825. [*Phil. Trans.* 1826, Part I. *p.* 1.]

The author prefaces these observations with a brief account of the instruments with which, and the circumstances under which, the observations previously communicated to this Society were made, and being fully described in the former paper alluded to in the title of this, require no further particular description; he contents himself therefore with noticing that by a different adaptation of their parts, higher magnifying powers than those formerly employed were obtained, and a series of powers from 92 to 787 used in a part of the observations.

A large portion of these observations were made at Passy near Paris, and the author takes occasion to make honourable mention of the facilities afforded him on the part of the French Government for the ingress and egress of his instruments into and out of France, and of the attention and assistance uniformly afforded him while resident there by many distinguished individuals.

Of the stars whose measures are here presented, he states that about 160 are hitherto undescribed and probably new. The places of these are given merely with sufficient exactness to enable any one to find them in future. The remainder are in great measure stars comprised in Mr. Struve's catalogue of 796 double and triple stars, and among them about 160 belong to those examined for the first time by Sir William Herschel.

The observations themselves are stated in a manner somewhat different from that adhered to in the former communication already alluded to. Instead of giving all the individual micrometrical measurements on which they depend (about 14,000 in number,) which would have swelled the paper to an enormous bulk, only the mean results of each set of measures are given: but to afford every opportunity of forming an impartial judgement of their validity, not only the number of measures on which it depends is annexed to each mean

result, but also the difference between the greatest and least means taken, or the limits within which all the measures necessarily lie.

The stars themselves are arranged in order of right ascension for convenience of reference. After the statement of the mean results of the several sets of observations both of angle and distance, a final mean with a mean date for an epoch is deduced. In the case of Sir William Herschel's stars, a comparison of the measures now obtained with those given in his catalogues, or now for the first time brought to light by a careful examination of his manuscripts, is subjoined. By this comparison several fresh instances have been found of double stars, in which the relative motion of the individuals composing them is satisfactorily proved. In one remarkable case (that of the star δ Equulei,) this change has gone to an enormous extent, and is satisfactorily referred to proper motion in the large star. In another not less singular, all the three stars of a triple star (ζ Cancri) are ascertained to be relatively in motion, describing orbits about each other, and forming probably a ternary system by the mutual gravitation of its members, thus completely justifying the views taken by Sir William Herschel of this subject, in his papers published in the Transactions of this Society in 1802 and 1804.

Annexed, as an appendix to these observations, is a re-examination of about 27 stars measured in the former paper already alluded to, and which were considered as presenting peculiar interest, from the evidence then obtained of their relative motion and of their connexion in binary systems. The results of this re-examination are in the highest degree satisfactory, as, with only two or three exceptions, these stars have been found to continue their motions in the directions, and in the greater number of cases with nearly the velocities, predicted. In the most remarkable case, that of the double star ξ Ursæ Majoris, an angle of nearly 14° has thus been described by the two stars about their common centre of gravity in an interval of less than two years, thus affording every probability that in a very few years we shall arrive at a perfect knowledge of the figure, elements, and position of their orbits, and be enabled by strict calculation to answer the important question, whether the Newtonian law of attraction is confined to our own system or obtains also in the sidereal heavens.

An Account of the Construction and Adjustment of the New Standards of Weights and Measures of the United Kingdom of Great Britain and Ireland. By Captain Henry Kater, *F.R.S.* Read November 24, 1825. [*Phil. Trans.* 1826, Part II. *p.* 1.]

The author, after stating that the weights and measures of the United Kingdom are founded on a standard whose length is determined by its proportion to that of a pendulum vibrating mean time in London, which has been ascertained by him to be 39·13929 inches of Sir George Shuckburgh's scale, considers it necessary, on account of the importance of the result, to consider what degree of confidence

it is entitled to. For this purpose it is necessary to compare this final result with those obtained in other experiments and by different methods. Now it appears that previous to the experiments detailed in the author's paper on the subject in the Philosophical Transactions for 1818, on which this result rests, another series is there mentioned, made with the same instruments, but under circumstances which occasioned their rejection, and which, owing to some repairs in the instruments between the two series, which occasioned a material alteration in the distance between the knife edges, have all the weight of experiments made with a different pendulum; the result of these rejected experiments, however, differed only two ten-thousandths of an inch from that ultimately adopted.

The author next compares the length of the seconds pendulum at Unst and at Leith Fort, as ascertained by him by an invariable pendulum, whose vibrations had previously been determined in London, and whose length was thus known in terms of the London Seconds Pendulum, and as ascertained by M. Biot at the same stations, by means of a variety of pendulums, and by a totally different method of observation,—that of Borda. The results of this comparison are a difference between the determinations of M. Biot and the author, of 0·00029 inch in excess at the former station, and 0·00015 in defect at the latter.

From this near agreement of all the results, he considers that the length of the seconds pendulum in London, may be regarded as certainly known to within one ten-thousandth of an inch; while from the near agreement of the results of the French and English experiments on the length of the pendulum, he concludes that the length of the metre in parts of Sir George Shuckburgh's scale, may also be regarded as known within one ten-thousandth of an inch.

From an account recently published by Captain Sabine of his valuable experiments for the determination of the variations in length of the seconds pendulum, he observes, doubts may be inferred of the accuracy of the method employed by him for the observations for determining the length of the seconds pendulum in London, as well as in those which have been made with the invariable pendulum. It is asserted there, that taking a mean between the disappearances and re-appearances of the disc, is a more correct method of observation than than pursued by Captain Kater, and that the intervals between the coincidences obtained, by observing the disappearances only of the disc, would be productive of error.

In answer to this objection the author remarks, 1st, That with respect to the convertible pendulum, or that used for determining the absolute length of the seconds pendulum, the disc was made to subtend precisely the same angle as the tail-piece of the pendulum, so that at the moment of disappearance its centre necessarily coincided precisely with the middle of the tail-piece, and the difference between the moments of disappearance and re-appearance is rigorously nothing, an adjustment indispensable in his method of observing, when the object is to determine the true number of vibrations in 24 hours.

2ndly. With the invariable pendulum, the disc subtended a somewhat less angle than the tail-piece, so that the inferred number of vibrations in 24 hours was diminished about $\frac{1}{10}$ths of a second. But experiments with the invariable pendulum being intended to be in the strictest sense of the word comparative, this constant difference will no way affect the ultimate result. But as the most direct way to remove any doubts which may be entertained on the subject, the author has computed from the whole of Captain Sabine's observations, the successive differences in the vibrations at the various stations visited by him, by the two methods, viz. that of employing the disappearances and re-appearances, and the disappearances alone. The results only in one instance differ so much as a tenth part of a vibration. They are indifferently in excess and defect, and the mean of their discrepancies is exactly nothing. From this he concludes, that if the observations be made as nearly as possible under similar circumstances, the method of observing by disappearances alone is productive of no perceptible error in practice, in experiments with the invariable pendulum, while in those with the convertible pendulum the equal apparent size of the disc and tail-piece precludes the possibility of any, either in practice or theory, from this cause.

The standard of Sir George Shuckburgh having been found identical with that of Bird, in the custody of the clerk of the House of Commons, adopted as the imperial standard unit of extension, the length of the pendulum already determined is fixed with the same degree of precision in parts of the Imperial standard yard.

A repetition of Sir George Shuckburgh's experiments on the weight of given volumes of distilled water, and a re-measurement of the cube, sphere, and cylinder used by him, were found to give no material variation from his results; and these being stated in terms of the mean of several standard weights kept at the House of Commons, the troy pound nearest the mean has been adopted and declared by the legislature to be the original unit of weight under the denomination of the Imperial standard troy pound.

The relation between this pound and the cubic inch of distilled water at 62° Fahrenheit, barometer 30 inches, has been ascertained by the Commissioners of Weights and Measures, who find that the latter contains 252,458 grains, each grain being the 5760 part of the standard troy pound.

The avoirdupois pound is fixed for assigning its proportion to the standard troy pound, so as to contain exactly 7000 such grains.

The Imperial standard gallon is defined by stating its contents under the same circumstances of temperature and pressure at 10 pounds avoirdupois; and the bushel, by its containing 80 such pounds.

The author having, in compliance with a request of the Lords Commissioners of His Majesty's Treasury, undertaken to superintend the construction of, and to adjust the principal standards to be deposited at the Exchequer, Guildhall, Dublin, and Edinburgh, Mr. Dollond was directed to prepare those of linear measure, and Mr. Bate those of weight and measure, the proper quality of metal for

the latter purpose being determined by experiments instituted for the purpose.

The experiments for adjusting them are then given in full detail. The troy pounds were first adjusted, and the exactness with which this operation has been performed may be appreciated from this,—that the final errors of none of them exceed $\frac{21}{10000}$ths of a grain. When brought so near, it was of course not thought necessary to attempt further correction.

The avoirdupois pounds and the weights of the gallon of water were then derived from the troy pounds, and finally adjusted like them by inclosing within the weight, in hollows left for the purpose, wires equal to the errors ascertained to exist in them; the weights of these wires in each case is stated; so that should they by any accident be taken out and lost, they may be restored.

He next describes the method used in adjusting the gallon itself, the method of filling it exactly and of weighing it when filled, together with the corrections depending on the circumstances of temperature and pressure under which the experiments were made. As a final result, it appears that one only of the gallons was ultimately found in error to a greater extent than $\frac{8}{10}$ths of a grain, the others having their errors less than a fourth of that quantity.

The quarts and pints being next disposed of, the author describes the balance contrived by him for weighing the bushels, which proved so delicate as to turn with a single grain when loaded with 250 pounds in each scale. The resulting bushels when finally adjusted, were found to have all their apparent errors less than 6·56 grains of water, while the corrections for temperature and pressure only amounted in some cases to no less than 138 grains; but this depending on the figure of the glass used to cover them, it is not to be understood that the contents of the vessels have actually been ascertained to this degree of precision.

The adjustment of the standard yards is next described; and the author concludes his paper by a summary of the results arrived at in the present inquiry respecting British weights and measures. The length, he remarks, of the pendulum vibrating seconds in London has been found in parts of the Imperial standard yard; so that the value of the yard may at any time be known, having been referred to a natural standard presumed unalterable. The length of the French metre, a standard expressing a certain portion of the terrestrial meridian, has also been given in parts of the English scale. The weight of a cubic inch of distilled water has been determined in parts of the Imperial troy pound, and thus the pound if lost may at any future time be recovered. The avoirdupois pound is now for the first time defined, and the measures of capacity are made to depend on the weight of water they contain; the Imperial gallon containing ten pounds avoirdupois of water, having been declared to be the unit or only standard measure of capacity, from which all others are to be derived. This, it is presumed, will tend powerfully to produce uniformity throughout the United Kingdom, by putting

it in the power of every individual possessed of standard weights to verify his measures of capacity with the utmost facility.

Description of an improved Hygrometer. By Mr. Thomas Jones. *Communicated by Captain* Henry Kater, *F.R.S.* Read June 16, 1825. [*Phil. Trans.* 1826, Part II. *p.* 53.]

The principle of Mr. Jones's Hygrometer is essentially the same with that of Mr. Daniell's, or rather with that employed by Mr. Dalton to determine the quantity of aqueous vapour present in the air, viz. to ascertain the temperature at which dew is deposited from the atmosphere. It differs from Mr. Daniell's, however, in the frigorific action being applied *immediately* to the bulb of the thermometer employed to measure the temperature.

This bulb is of a considerable size, and of a cylindrical form, slightly flattened, and extended at the end. The stem of the thermometer being twice bent at right angles, this end of the bulb turns upwards. It is made of black glass and is exposed, but the rest of the bulb is covered with muslin. This being moistened with ether, the mercury is cooled, and dew at length settles on the exposed part, at which moment it is read off.

Mr. Jones, after describing this instrument, obviates an objection to its use, drawn from the application of the frigorific process to the *lower* part of the bulb, while the dew is deposited at the upper. This objection, if valid, might be obviated, by inclining the bulb so as to have its axis horizontal. But repeated trials have satisfied him of there being no occasion for this precaution.

Mr. Jones finally alludes to the use of a similar construction in Vienna.

Observations on the Changes which have taken place in some ancient Alloys of Copper. By John Davy, *M.D. F.R.S. In a Letter to Sir* Humphry Davy, *Bart. P.R.S.* Read November 17, 1825. [*Phil. Trans.* 1826, Part II. *p.* 55.]

Dr. Davy first describes the nature of an incrustation upon an ancient helmet found in a shallow part of the sea, between the citadel of Corfu and the village of Castrades. The surface was of a variegated colour, mottled with spots of green, dirty white, and red. The red and green patches exhibited minute crystals of red oxide of copper, and metallic copper; and were further composed of its green submuriate and carbonate. The dirty white parts consisted chiefly of oxide of tin. These new combinations are only superficially produced; the metal was found bright beneath, and consisted of copper alloyed with 18·5 parts of tin.

An ancient nail from a tomb in Ithaca, and a mirror from a tomb at Samos, in Cephalonia, afforded nearly similar but less distinctly crystalline results. The copper in the mirror was alloyed with 6 per cent. of tin, and a minute portion of arsenic.

The examination of the incrustation upon ancient coins showed it to consist of oxide of tin, and of carbonate and submuriate of tin, and of carbonate and submuriate of copper. It in some cases acquires a dingy hue from the prevalence of black oxide of copper, mixed with a little of its protoxide.

The author could discover no connexion between the perfect state of preservation of ancient coins, and their composition; but he observes that the manner in which the crystalline structure of the incrustation is acquired is a peculiarly interesting question. There being no reason to suspect deposition from solution, "are we not," says the author, "under the necessity of inferring, that the mineralizing process witnessed in its effects, depends on a slow motion and separation of the particles of the original compound? And must we not conclude that this motion is connected with the operation of attractions of different kinds, as chemical affinity, electro-chemical attraction, and attraction of aggregation?" If this conclusion be just, the author remarks that it opens a new field of inquiry, which may help to explain several phenomena in mineralogy and geology.

Additional Proofs of Animal Heat being influenced by the Nerves. By Sir Everard Home, *Bart. V.P.R.S.* Read November 16, 1825. [*Phil. Trans.* 1826, Part II. *p.* 60.]

In this communication Sir Everard gives an account of a repetition of his former experiments upon the effect of dividing the nerves that supply the velvet of the deer's horns, and in which some sources of error have been avoided. The general results are the same as those formerly obtained; the temperature of the horn, the nerves of which were divided, was diminished to the amount of 7°; and, as before, the disparity of temperature gradually decreased until, after the lapse of about twelve days, the temperatures of the two horns were the same. Upon examining the structure of the parts after the animal's death, it was found that the interval between the divided ends of the nerves was filled up by a newly-formed connecting substance, capable of restoring their action.

In further illustration of the effect of the nerves in producing heat, independent of mere circulation of blood, the author mentions a case of aneurism, in which the femoral artery was tied without occasioning any diminution in the temperature of the foot.

The Croonian Lecture. On the Structure of a muscular Fibre from which is derived its Elongation and Contraction. By Sir Everard Home, *Bart. V.P.R.S.* Read December 15, 1825. [*Phil. Trans.* 1826, Part II. *p.* 64.]

In this paper, after attending to the striking analogy between the structure of a nervous and of a muscular fibre, as demonstrated by Mr. Bauer's microscopical observations, Sir Everard adverts to an error into which he had fallen, in his former examination of the

muscle, depending upon its having been previously boiled, by which the globules were altered, their colouring matter separated, and any connecting medium between them destroyed; in such case, therefore, the skeleton only of the muscular fibre remained.

The muscular fibre which was now selected for examination, was taken from the fasciculi composing the great muscle that lies upon the back of the bullock's neck; it was examined within twenty-four hours after the animal's death. By immersion in water, an integral fibre was separated for inspection in the field of the microscope. Its mechanism corresponded with that of the nervous fibre of a ganglion, but the globules were larger in the proportion of [illegible] to [illegible] and [illegible] parts of an inch.

The gelatinous matter by which the globules adhere together is less elastic than in the nervous fibre, so that the muscular fibre could not be extended to double its length without breaking.

The muscle of a trout exhibited the same appearance as that of the bullock's neck, but the fibres were more brittle.

From the facts stated in this lecture, together with those formerly adduced respecting the structure of ganglions and nerves, Sir Everard observes, that they agree with muscles in consisting of single rows of globules united by a transparent elastic gelatinous matter; the globules, however, differ in size, and the elastic medium is more easily elongated, and restores itself more readily in a nerve than in a muscle. An illustrative drawing accompanies this lecture.

An Account of the Heat of July, 1825; *together with some Remarks upon sensible Cold. By* W. Heberden, *M.D. F.R.S.* Read January 12, 1826. [*Phil. Trans.* 1826, Part II. *p.* 69.]

The temperatures which Dr. Heberden wishes to record in this paper, are those observed on the 15th, 17th, 18th, and 19th of last July, and were respectively 92°, 90°, 96°, and 95°. On the 15th, the wind was S.W., on the other days it blew from the East. The thermometer employed was sensible and accurately graduated, and was suspended upon a lawn, about 5½ feet from the ground; on the first day, in the shade of a laburnum tree, and afterwards from an external branch of a large Portugal laurel; always distinct from any building; exposed to the full influence of the wind, and at the same time sheltered from the actual rays of the sun, and from substances heated by them. The author adds, that the only instance on record of a corresponding elevation of atmospheric temperature was in July, 1808; on the 13th of which month, it appears from the Royal Society's register, the thermometer rose to 93°·5, and Mr. Cavendish's thermometer, at Clapham, to 96°. By way of comparison, Dr. Heberden observes, upon the authority of the late Dr. Hunter, that in the hottest season, and during the hottest part of the day, the range of the thermometer, at Kingston, in Jamaica, is from 85° to 90°.

To these remarks, Dr. Heberden adds some observations on the imperfection of the thermometer, as a measure of the degree of cold

perceptible to the human body in its ordinary exposure to the atmosphere, and which depends upon the rapidity with which its own heat is carried off by the conducting power and currents of the atmosphere. To estimate this insensible cold, the author raised the thermometer to 120°, and then carried it into the open air. As soon as the mercury had fallen to 100°, the rate of its further descent, during every 10″, was noted for half a minute, in different states of the atmosphere in regard to wind and moisture. These experiments, which are given in the form of tables, show the powerful effect of wind in increasing the rate of cooling, and consequently of exciting the sensation of cold in the human body, independent of any actual low temperature of the atmosphere.

On the Transit Instrument of the Cambridge Observatory; being a Supplement to a former Paper. By Robert Woodhouse, *Esq. Plumian Professor of Astronomy in the University of Cambridge.* Read January 19, 1826. [*Phil. Trans.* 1826, Part II. *p.* 75.]

This communication is intended merely as a supplement to a former paper on the same subject, printed in the Transactions of this Society, in which a deviation of the transit instrument from the plane of the meridian, arising from a difference of expansion in its braces, was pointed out. As no instance was there given of the magnitude of this deviation, one is here adduced in which the inferior passage of the pole star was found to have been retarded twenty-five seconds in consequence of the sun having been allowed to shine on the upper western brace, the object-glass of the transit being towards the zenith. The author adds, that in consequence of the detection of this source of inequality, he now views with great suspicion all his previous observations of solar transits.

Account of a Series of Observations, made in the Summer of the Year 1825, *for the purpose of determining the Difference of Meridians of the Royal Observatories of Greenwich and Paris; drawn up by* J. F. W. Herschel, *Esq. M.A. Sec. R.S. Communicated by the Board of Longitude.* Read January 12, 1826. [*Phil. Trans.* 1826, Part II. *p.* 77.]

The operations, of which this paper contains an account, were undertaken by the British Board of Longitude, in conjunction with the French Ministry of War, at the invitation of the latter, for the purpose of connecting the Royal Observatories of Greenwich and Paris, by means of signals contemporaneously observed along a chain of stations established for that purpose between them. The signals employed were the explosions of proper quantities of gunpowder, elevated to a great height in the air by rockets fired at three stations, two on the French, and one on the English side of the Channel, and observed at the observatories, and at two stations intermediate between those at which they were fixed. These two stations

were Lignieres on the French, and Fairlight Down, near Hastings, on the English side. The operations at the former were made by Captain Sabine and Colonel Bonne; those at the latter by Mr. Herschel and Mr. Largeteau; thus securing two independent lines of communication, a British and a French, between the extreme stations, and observers at each station exchanging observations at the termination of the whole operation, which was continued for twelve nights, ten signals being made at each station per night; and though the whole of them could not be employed, it is stated that the final result, which makes the difference of longitude between the two observatories equal to 9′ 21″·6, is not very likely to be found one tenth of a second in error, and extremely unlikely to prove erroneous to twice that amount.

The observations are then stated in detail; and the mode of combining them in the most impartial manner, so as to deduce from them the most advantageous result, is next investigated; and a general formula deduced applicable to all operations of the kind, and including all the necessary corrections. This formula is then applied to the actual observations, and the result above mentioned deduced. The two results deduced by considering separately the observations of the British and of the French observers, at the intermediate stations, exhibit a remarkable coincidence,—their difference amounting to only a single hundredth of a second.

Observations on the Poison of the Common Toad. By John Davy, *M.D. F.R.S.* Read December 22, 1825. [*Phil. Trans.* 1826, Part II. *p.* 127.]

After adverting to the correctness of the popular opinion respecting the poisonous nature of the toad, which the professed naturalist has generally rejected, the author proceeds to describe the seat of the poison, which is chiefly in follicles in the cutis vera, and which, on pressure, exude from it in the form of a thick yellowish fluid, which, on evaporation, yields a transparent residue, very acrid, and acting on the tongue like extract of aconite. It is neither acid nor alkaline; and since a chicken inoculated with it received no injury, it does not appear to be noxious when absorbed and carried into the circulation. Indeed, although it chiefly abounds in the integuments of the toad, the author also detected it in the bile, the urine, and in small quantity in the blood.

Dr. Davy thinks that the principal use of this poison is to defend the reptile against the attacks of carnivorous animals; he also remarks, that as it contains an inflammable substance, it may be considered as excrementitious; it may serve to carry off a portion of carbon from the blood, and thus be auxiliary to the function of the lungs. In support of this idea, the author observes that he finds each of the pulmonary arteries of the toad divided into two branches, one of which goes to the lung, and the other to the cutis, ramifying most abundantly where the largest venous follicles are situated, and

where there is a large venous plexus, corresponding to the site of the tadpole's gills, and seeming to indicate that the subcutaneous distribution of the second branch of the pulmonary artery may further aid the office of the lungs by bringing the blood to the surface to be acted on by the air.

The author concludes this paper by stating, that he has endeavoured to trace a direct communication by spiracula through the integuments, but has only obtained negative results, never having succeeded in forcing the smallest bubble of air through the skin.

On the magnetizing Power of the more refrangible Solar Rays. By Mrs. M. Somerville. *Communicated by* W. Somerville, *M.D. F.R.S. February* 2, 1826. Read February 2, 1826. [*Phil. Trans.* 1826, Part II. *p.* 132.]

In the year 1813, Professor Morichini, of Rome, announced that steel exposed in a particular manner to the concentrated violet rays of the prismatic spectrum becomes magnetic. His experiments, however, having uniformly failed in other hands, had ceased to excite general attention; especially in this country, whose climate is usually so unfavourable for such researches. The unusual clearness of weather last summer, however, induced Mrs. Somerville to make the attempt. Having, at that time, no information of the manner in which Prof. Morichini's experiments were conducted, it occurred to her, however, as unlikely that if the whole of a needle were equally exposed to the violet rays, the same influence should at the same time produce a south pole at one end, and a north at the other of it. She therefore covered half of a slender sewing needle, an inch long, with paper, and fixed it in such a manner as to expose the uncovered part to the violet rays of a spectrum, thrown by an equiangular prism of flint glass on a panel at five feet distance. As the place of the spectrum shifted, the needle was moved so as to keep the exposed part constantly in the violet ray. The sun being bright, in less than two hours the needle, which before the experiment showed no signs of polarity, had become magnetic; the exposed end attracting the south pole of a suspended magnetic needle, and repelling the north. No iron was near to disturb the experiment, which was repeated the same day, under similar circumstances, with a view to detect any source of fallacy in the first attempt, but with the same result.

The season continuing favourable, afforded daily opportunities of repeating and varying the experiment. Needles of various sizes (all carefully ascertained to be free from polarity), and exposed in various positions with regard to the magnetic dip and meridian, almost all became magnetic; some in a longer, some in a shorter time, varying from half an hour to four hours, but depending on circumstances not apparent. The position of the needles seems to have had no influence, but the experiments were generally more successful from 10 to 12 or 1 o'clock than later in the day. The exposed portion of the needle became (with a few exceptions) a north pole,

exceptions possibly attributable to some predisposition in the needle itself to magnetism too slight to be observed. The distance of the needle from the prism was varied without materially varying the effect. It was found unnecessary to darken the room, provided the spectrum was thrown out of the direct solar rays.

The next object was to ascertain whether the other prismatic colours had the same property as the violet. Needles (previously ascertained to be unmagnetic) exposed to the blue and green rays sometimes acquired magnetism, though less frequently, and requiring longer exposure; but the magnetism *when communicated* seemed equally strong as in the case of the violet rays. The indigo succeeded nearly as well as the violet. In all cases, the exposed end usually became a north pole. But *in no one instance was magnetism produced by the yellow, orange, or red rays*, though in some cases the same needles were exposed to their influence for three successive days, neither did the calorific rays produce any effect.

Pieces of clock- and watch-spring were next tried with similar success, and they were even found more susceptible of the peculiar magnetic influence than needles; possibly from their greater proportional surfaces, or their blue colour. The violet rays concentrated by a lens produced magnetism in a shorter time than the prism alone.

Experiments were next instituted by transmission of the solar rays through coloured media. Three needles, half covered with paper as before, were exposed on a stone outside a window, under a blue glass coloured by cobalt, to a hot sun for three or four hours. They were rendered feebly magnetic, and when examined next day were found to have lost their magnetism; a circumstance found occasionally to take place afterwards, as the season advanced and the force of the sun diminished. No iron was near, nor did the stone show any signs of influence on a suspended magnetic needle. The experiment was repeated next day, leaving two needles six hours exposed, when the magnetism communicated was found to be very sensible, and is still retained by them at the distance of six months.

Pieces of clock-spring which had been heated to deprive them of magnetism also became magnetic when so exposed. The rays transmitted through the glass employed in this experiment, blackened muriate of silver as powerfully as those transmitted through uncoloured glass, proving its free permeability to the chemical rays.

Green glass was next tried, and the rays which penetrated it were found to communicate magnetism.

Neutral pieces of watch-spring were then half exposed (when wrapped in violet and green ribands, and fastened inside of the frame of a window,) to the sun's rays all day. In the evening both had become magnetic, and the parts which received the rays transmitted through the ribands became north poles.

Similar pieces were then freely exposed (half covered as all along with paper) to the white light of the sun, with neither prism glass nor riband; and though the heat was greater than before, no mag-

netism was produced. The same result was afterwards obtained in comparative trials; the steel was found to be no way affected by white light.

As the season advanced and the sun's force diminished, these effects became more feeble, and further experiments were in consequence deferred till the return of summer; but from the experiments here detailed, the authoress thinks herself entitled to regard a magnetic influence in the more refrangible solar rays as demonstrated.

On the mutual Action of Sulphuric Acid and Naphthaline, and on a new Acid produced. By M. Faraday, *F.R.S. Corresponding Member of the Royal Academy of Sciences, &c. &c. Communicated January* 12, 1826. Read February 16, 1826. [*Phil. Trans.* 1826, Part II. *p.* 140.]

In this communication Mr. Faraday shows that when sulphuric acid and naphthaline act upon each other, a peculiar compound possessed of distinct acid characters is the result. This acid is most readily obtained by heating two parts of naphthaline with one of sulphuric acid. The mixture concretes on cooling, and separates into two parts, the uppermost of which is little else than naphthaline, but the lower, heavier part contains the peculiar acid, which, being soluble in water, is easily separated by that fluid, not, however, pure, but still containing mixed sulphuric acid. The author, however, obtained the pure acid by decomposing its compound with baryta, which is soluble, by sulphuric acid. It then had a bitter sour taste, and formed a distinct class of salts with the different bases, all of which are soluble in water and in alcohol, and combustible.

By careful evaporation of the aqueous solution of this acid, a white crystalline deliquescent solution was obtained, evolving water when heated, and at high temperatures affording sulphurous acid, charcoal, and naphthaline. To determine the ultimate component parts of this acid, its compound with baryta was subjected to rigid analysis; the results of which were

78 baryta	=	1	proportional.
80 sulphuric acid	=	2	proportionals.
120 carbon	=	20	——
8 hydrogen	=	8	——

This acid, therefore, only possesses half the saturating power of sulphuric acid, and it would accordingly appear that the hydrocarbon acts the part of a neutralizer of one of the proportionals of sulphuric acid. This property of hydro-carbon, the author observes, was pointed out to him by Mr. Hennell, as manifested in the formation of sulpho-vinous acid, before he had established it in regard to the above peculiar combination.

Mr. Faraday proposes to call the acid, of which he has described the sources, nature, and properties, Sulpho-naphthalic Acid.

On the Nervous Circle which connects the voluntary Muscles with the Brain. By Charles Bell, *Esq. Communicated by the President, January* 25, 1826. Read February 16, 1826. [*Phil. Trans.* 1826, Part II. *p.* 163.]

The author's object in this communication is to show that every muscle is supplied with two nerves of different properties, and that where nerves of different functions have a separate origin, and run a different course, two nerves must unite in the muscle in order to perfect the relation betwixt the brain and those muscles.

Referring to his former observations, Mr. Bell remarks, that when he had distinguished two classes of nerves going to the face, and had deprived the muscles of motion by dividing the nerve, a question naturally suggested itself as to the use of the remaining nerves, more especially on finding that the 5th pair, or *sensitive* nerve, was more profusely distributed to the muscles than to the skin, although they are found in surgical operations by no means to possess that exquisite sensibility which such abundance of nerves would appear to indicate.

The lower maxillary nerve, which is a branch of the fifth pair, is composed of a nerve of sensation and a nerve of motion, arising in two sorts, one the sensitive, the other the muscular. On the former division the Gasserian ganglion is formed, but the motive nerve may be traced clear of the ganglion to the muscles of the jaws. Now if all that is necessary to the action of a muscle be a nerve to excite contraction, these branches, says the author, should have been unaccompanied; but, on the contrary, they are joined before they enter the muscles by the sensitive nerves of the ganglion.

These and similar facts and observations lead Mr. Bell to ask why nerves of sensation are thus profusely given in addition to their motive nerves; and in the progress of this inquiry, he shows that a consciousness of the state and degree of action of the muscles is necessary to the governance of the muscular frame; that motive nerves are not those by which such information is conveyed to the brain, for they are concerned in carrying the influence of the will to the muscle; and it is not likely that the same nerve should be active in two directions at the same moment; for, without reference to the cause, a simple nerve has the influence propagated along it in one direction only, and cannot be shown to act both from and to the sensorium, as may be proved by actual experience, and in illustration of which, Mr. Bell refers to the effects of sundry nerves, and to certain cases of their morbid affections.

The author, therefore, concludes that between the brain and muscles there is a circle of nerves, that one nerve conveys the influence of the brain to the muscle, and that another gives the sense of the condition of the muscle to the brain. If this circle be broken by the division of the motive nerve, motion ceases; if it be broken by the division of the other nerve, there is no longer a sense of the condition of the muscle, and therefore no regulation of it actively.

Mr. Bell concludes this paper with some remarks upon the use of the plexus formed on both sets of nerves, and on their association upon the integuments. In regard to the plexuses, he considers them as concerned in associating the functions of distinct muscles; and in reference to the surface of the body, he remarks, that although the principal office of its nerves is to convey impressions to the sensorium, yet, on the other hand, the condition of the mind is often forcibly communicated to the skin. Hence the striking union of the branches of the fifth pair with the portio dura of the seventh pair in the integuments of the head and face.

On the Constitution of the Atmosphere. By John Dalton, *Esq. F.R.S. &c. Communicated January* 12, 1826. Read February 24, 1826. [*Phil. Trans.* 1826, Part II. *p.* 174.]

The object of this paper is to examine the consequences as respects the proportion of the component parts of the atmosphere simultaneously existing at different heights in one vertical column, which would follow from the atomic theory, on the supposition of a finite number of atoms existing in corporeal bodies, and of such a law of repulsion prevailing among those of elastic fluids, as Sir Isaac Newton appears to have supposed, in which the repulsive power of each particle terminates at the particles immediately adjacent. It is well known that when two or more mutually inactive gaseous fluids are mixed, each distributes itself uniformly through the whole space occupied, and each sustains a part of the whole pressure retaining them, proportioned to its density. This is a necessary consequence of the mutual inelasticity and independence of the gaseous atmospheres with respect to each other. Each exerts the whole mechanical force its quantity will allow, without regard to the others; and the sum of all these forces in the state of equilibrium counterbalances the total pressure.

This uniformity of density, however, is only a consequence of the assumed principle, where the gases occupy such small spaces as we can command in our experiments, in which the total pressure may be regarded as uniform, in a vertical as well as in a horizontal direction; it is otherwise when we regard a column of indefinite height, or one prolonged to the limit of the atmosphere,—a limit at which the weight of a single particle is in exact equilibrio with the repulsion between two contiguous ones. It is this case which the author considers in the paper before us. He supposes, for simplicity, two atmospheric columns, one of hydrogen, and the other of carbonic acid, each supporting at its base a pressure of 30 inches of mercury; of such height as to reach to the respective limits of each atmosphere, divided each by partitions into cells of equal magnitude, at first insulated from each other, then made to communicate, and finally, the cells to be withdrawn, and a free communication established between every part of the two columns: and from an analysis of what passes in the act of communication, and from the general principles

of pneumatic chemistry, he is led to the conclusion, that the arrangement of each of the gases in the united column will be precisely the same as if the other had no existence; that is, that each will form a separate and independent atmospheric column, containing at its base a pressure of fifteen inches, and decreasing in density according to its own peculiar law; so that after a certain height the limit of the carbonic acid atmosphere being passed, hydrogen alone would exist in the column, and after the limit of the hydrogen atmosphere were attained a vacuum.

The author takes occasion, in the course of this reasoning, to suggest, that the absolute height of an atmosphere of carbonic acid or other gas might be found by perfectly exhausting a tall receiver, then letting in a small given portion of the gas, and testing the upper and lower portions, to ascertain its presence below and absence above.

He terminates the paper, 1st, by a view of the constitution of the earth's atmosphere on this idea; according to which it appears, that the atmosphere of oxygen being supposed to reach to the height of 38 miles, that of azote will reach to the height of 54 or 44, according to the different assumptions which may be adopted respecting the weight of its atom;—that of carbonic acid to 10 miles, and that of aqueous vapour to 50 miles: and 2ndly, by a comparison of this view with what should be its constitution on the usual theory:—the whole of this view of course supposes the air to be perfectly at rest, but the actual constitution of the atmosphere is probably materially modified by the motions perpetually going on in it.

On the Coagulation by Heat of the fluid Blood in an aneurismal Tumour. By Sir Everard Home, *Bart. V.P.R.S. Communicated January* 23, 1826. Read March 2, 1826. [*Phil. Trans.* 1826, Part III. *p.* 189.]

In a case of aneurism of the external iliac artery, the author tied the femoral artery below the sac; but as this neither diminished the pulsation nor arrested the increase of the tumour, he was led to introduce a needle into its centre, connected with a heated bar of steel: as no severe pain was thus produced it was retained for fifteen minutes, during which the pulsation was diminished. On the twentieth day after the operation, the swelling having increased considerably, heat was applied as before; but by a larger needle, retained for thirty-five minutes, this tended to diminish the pulsation, but it returned on the third day with much pain in the tumour. On the forty-fourth day after the operation, a larger heated needle than the former was introduced for thirty minutes, when the pulsation suddenly stopped, and the patient felt free from pain. The tumour was solid to the touch, and there was no return of pulsation in it. The leg, however, soon afterwards became œdematous, mortification ensued, and the patient died on the ninetieth day after the operation, of the effect of the mere pressure of the tumour. The appearance of the coagulum found in the sac after death is shown in an annexed draw-

ing, and the author describes the general appearances of the diseased parts, among which ossification of the arterial trunks was prevalent. He trusts that he has proved that coagulation of the blood in an aneurismal sac, by the means pointed out in this paper, is not only practicable, but that it may be resorted to without the production of any important local or constitutional symptoms.

Sir Everard next details the results of several experiments made with a view of ascertaining the effects of various temperatures upon the spontaneous coagulation of the blood. The separation of serum he finds considerably impeded by a high temperature, but by a heat of 120° the blood is rendered buffy; and if drawn from the arm into a cup immersed in boiling water, and kept for some hours at that temperature, it does not form a complete coagulum. A low temperature also interferes with its perfect coagulation; for when drawn into a cup immersed in ice, and left there, in twenty-four hours the surface had a buffy coat, and the coagulum was extremely loose.

It has been generally believed that the cupped appearance of blood depends upon the coagulable lymph being more contractile when separated from, than when blended with, the other parts of the blood. In a patient who suffered under inflammation of the brain, and who was bled in the course of thirteen days to the amount of sixty-eight ounces, some of the blood, though very buffy, was not at all cupped. Its appearance was very peculiar, and the coagulum was divided into an upper dense portion, having the characters of coagulated albumen; while the lower portion had a gelatinous appearance, and exhibited the albumen in a very attenuated state, mixed with the colouring matter.

On the mathematical Theory of Suspension Bridges, with Tables for facilitating their Construction. By Davies Gilbert, *Esq. V.P.R.S. &c. Communicated March* 9, 1826. Read March 9, 1826. [*Phil. Trans.* 1826, Part III. *p.* 202.]

In this paper the author states that his attention was first directed to a consideration of suspension bridges, when the plan for the Menai Bridge was submitted to the Commissioners of Roads and Bridges. It then appeared to him that the proposed depth of curvature was insufficient for insuring a due degree of strength; and this opinion was confirmed by some investigations, which are printed in the Quarterly Journal of Science. In consequence of this, the interval between the road-way and the points of support has been augmented to 50 feet, and its strength now appears sufficient.

The object of this paper is the expansion of the formulæ, from which the above-mentioned approximation was derived, into tables adapted to general use; and the derivation of other formulæ and tables for the catenary of equal strength; a curve not merely of speculative curiosity, but of practical use when bridges of very wide span are to be constructed. The author first remarks, that as all catenaries,

like circles, parabolas, &c., are similar curves; tables constructed for one value of the parameter apply to all by simple proportion.

He then enters into an analytical investigation of the equations suited for his purpose, and finally concludes his paper with four tables. The first consists of six columns, and contains corresponding values of the parameter; the exponential function or inverse logarithm of the span, divided by the parameter; the versed sine, the length, the tension at the point of support, and the angle of suspension;—all computed for a semi-span of 100 parts, for the ordinary catenary, and for a parameter varying from 1000 to 2000.

Table 2. contains the same quantities for a constant parameter 100, and for all values of the semi-span from 1 to 100.

Table 3. is adapted to the catenary of equal strength, and corresponds to Table 1. in the ordinary catenary; the constant span being 100, and the parameter varying from 70 to 1000, with an additional column expressing the weight of the whole curve; while Table 4. exhibits the same things for this curve, arranged in the order of Table 2.

By the aid of these Tables all the requisite particulars may easily be found in any case proposed in practice.

On Magnetic Influence in the Solar Rays. By Samuel Hunter Christie, *Esq. M.A. F.R.S. of Trinity College, Cambridge; Fellow of the Cambridge Philosophical Society: of the Royal Military Academy. Communicated November* 15, 1825. Read January 19, 1826. [*Phil. Trans.* 1826, Part III. *p.* 219.]

The object of this communication is to show, by a series of experiments, that the sun's rays possess sensible magnetic properties, which are observable in the vibrations of a magnetic needle exposed to them independent of their heating effect; and also to point out the changes which take place in the intensity of a needle's magnetism, from changes of temperature, as deduced from its times of vibration.

The main fact noticed by the author, indicative of a magnetic influence in the solar rays, is this;—that a magnetic needle vibrating, exposed to the rays of the sun, comes to rest more quickly than when vibrating in the shade.

A needle, six inches long, contained in a brass compass-box with a glass cover, was suspended by a fine hair, and made to vibrate, alternately shaded and exposed to the sun. The shade was produced by a wooden screen, supported four feet above the box. It was then found that (setting off from the same point,) the 100th vibration could be very distinctly noted in the shade, but none further than the 75th in the sun. So far, too, from the increase of temperature in the needle having caused the vibrations to be performed slower in the latter case, they were actually executed somewhat more rapidly.

In another experiment a heavier needle was suspended by a fine wire, and when heated by exposure to the sun its decrease of intensity was ascertained by torsion of the wire, and was found to correspond nearly with the author's previous determinations; but the terminal arc, after fifty vibrations made in the sun, was found always considerably less than after the same number in the shade, the initial arcs being the same in both cases. Other observations made in strong sunshine, and of which a detail is given, led to the same conclusions; the terminal arc in the shade, after forty vibrations, being 14°, and in the sun only 8½, the initial arc being 90°.

That this effect does not arise from change of temperature and intensity in the needle is evident from the observations themselves. To show that it does not arise from change of temperature in the brass box, the author heated the box over a fire till its heat was barely supportable by the hand; and the needle being vibrated alternately in the box so heated, and in the cold box (but in neither case exposed to the sun), the effect of increased temperature was found decidedly and considerably the reverse of that of the solar radiation, the terminal arc being materially increased by the heat,—a circumstance, he thinks, indicative of a diminished capacity for magnetism in brass at an elevated temperature.

The author next tried the effect of an elevation of temperature in the needle itself, by dipping it in boiling water, but found no sensible effect on the terminal arc.

The small accelerations in the times of vibrations in the experiments first described, the author attributes to the diminution of the arcs. The first observations in which the peculiar effect was noticed were made June 4, 1824; and he regrets that his absence from home during the hot and clear weather of the summer of 1825, prevented his extending the inquiry by further and obvious experiments. Meanwhile he regards these observations as tending considerably to remove the doubts raised respecting the influence of the violet ray in Professor Morichini's experiments, arising from their repeated failures in the ablest hands.

On the mutual Action of Sulphuric Acid and Alcohol, with Observations on the Composition and Properties of the resulting Compound. By Mr. Henry Hennell, *Chemical Operator at Apothecaries' Hall. Communicated by* W. T. Brande, *Esq. Sec. R.S.* Read March 9, 1826. [*Phil. Trans.* 1826, Part III. *p.* 240.]

At the commencement of this paper Mr. Hennell describes certain peculiarities in the properties of oil of wine, which induced him to consider sulphuric acid as one of its proximate elements; and on following up his analytical experiments upon it he found that about 37 per cent. of that acid might be obtained during its decomposition, although in its original state it affords no indications of that acid by the tests of the soluble salts of baryta,—a circumstance which he refers to the presence of hydrocarbon exerting a peculiar saturating

influence upon the acid. Of this hydrocarbon he next determines the composition, and finds that its elements correspond in their relative proportions with those of olefiant gas.

When oil of wine is mixed with solution of muriate of baryta, and gently heated, the mixture becomes acid, reddening litmus paper, but yet does not precipitate the barytic salt. Several experiments are detailed illustrating the nature of this acid, from which it appears that it forms very soluble compounds with baryta and potassa; the latter is a crystallizable salt, which burns with flame when heated, and leaves a bisulphate of potassa. Its analysis, the details of which are given at length in the paper, shows it to consist of two proportionals of sulphuric acid, one of potassa, four of carbon, and four of hydrogen; and it is remarked that the latter elements, namely, the carbon and hydrogen, appear in the present instance to be equivalent to, or to exert a saturating power over, one of the proportionals of sulphuric acid. Some slight discrepancies between the experimental and theoretical results of these analyses are adverted to, which the author thinks himself justified in attributing to water of crystallization in the salt, which he could not succeed in obtaining in a perfectly anhydrous state.

Mr. Hennell next shows that the salts, called Sulphovinates, are not essentially different from those which he has just described, and that they are not, as some have supposed, hyposulphates, modified by the presence of essential oil. In preparing the sulphovinates he was struck with the singular change effected upon sulphuric acid, by mixing it with its weight of alcohol. A portion of sulphuric acid, adequate to the saturation of 555 grains of carbonate of soda, required only 398 grains for its saturation when previously mixed with alcohol; and again a quantity of sulphuric acid, which afforded 1313 grains of sulphate of lead, only produced 542 grains when it had been mixed with its weight of alcohol. These circumstances are referred to the combination of a portion of the sulphuric acid with hydrocarbon derived from the alcohol.

Some experiments are then detailed, having for their object the separation of the hydrocarbon from oil of wine. When this oil, as it is called, is heated with a little potash, the salt above described is formed, and the excess of hydrocarbon is liberated in the form of a thick oil, which crystallizes at low temperatures: it has an aromatic odour, sp. gr. 9, is insoluble in water, but soluble in alcohol and ether; decomposition by peroxide of copper showed it to consist of carbon and hydrogen in the proportions of 6 and 1,—analogous therefore, as far as its ultimate elements are concerned, to olefiant gas.

The author examined some sulphuric acid given to him by Mr. Faraday, which had been made to absorb about 80 volumes of olefiant gas; and this saturated with carbonate of potash, evaporated to dryness, and the residue, treated by alcohol, afforded a portion of the same salt as that obtained from the oil of wine. The author concludes, therefore, that hydrocarbon, composed of single proportionals of its elements, is capable of entering into a peculiar

neutral combination with sulphuric acid, and that the compound in its purest known form constitutes what has been called *oil of wine*; and that when in this state it is acted upon by certain salifiable bases, a portion of the hydrocarbon is thrown off, and a distinct set of neutral salts formed, which are resolvable by heat into bi-sulphates; and which therefore include *two* proportionals of the elements of sulphuric acid, *one* of proto-carburet of hydrogen, and one of base.

On a Method of expressing by Signs the Action of Machinery. *By* Charles Babbage, *Esq. F.R.S. Communicated January* 17, 1826. Read March 16, 1826. [*Phil. Trans.* 1826, *p.* 250.]

In the construction of an engine for calculating and printing mathematical tables, in which the author of this paper has been for some time occupied, he states himself to have met with considerable difficulty from the want of any method by which all those motions which take place in any machine at the same instant, may be easily perceived and referred to, and by which the movement of any part might readily be traced back, through all the intervening stages, up to the first mover of the machine. The usual modes of mechanical drawing he found quite insufficient for these purposes, except in machinery of the simplest construction; and, even if they had not altogether failed in more complicated cases, the time and expense required for their execution would have effectually prevented their employment.

The most important question was to contrive some method by which all the simultaneous movements, occurring at any moment, should be at once visible; and the history of the state of motion or rest of any given part should be apparent during the whole cycle of the action of the engine. The author had therefore recourse to a system of signs, which bear an analogy to those employed in algebra, whilst they differ from them by having a general resemblance to the things they are intended to represent. Having gradually found that this system, which he calls "mechanical notation," was readily susceptible of affording other information than that for which it was at first contrived, he was led to give to it additional extension.

In its present form it gives, almost at a glance of the eye, information relative to any of the following points.

The names of every part of any engine being written at the top of the paper:—

1. Its representations in all the drawings will be pointed out.
2. The number of teeth in any wheel, pinion, or sector will be seen.
3. The actual angular velocity will also appear.
4. The mean angular velocity will also appear.
5. The origin of the motion of each part will be seen, and thus the cause of its motion will be traced up to the first mover.
6. At each transfer of movement, the method by which it was

accomplished will become apparent; whether by wheel and pinion, by a stud, by stiff friction, or by any other method.

7. All the adjustments which are necessary in order to set the machine in action will be pointed out, and the order in which they ought to be made will be indicated.

8. The whole course of action of every part will be visible in every stage of the progress of the machine. If it is a wheel, the time and direction of its motions will appear, and also the times at which it rests; if the part is a bolt or click, the times at which it is bolted or locked, and those during which it is in the reverse state, will be seen. These particulars will be discovered by casting the eye down the vertical line belonging to each part, which was named at the top of the drawing.

9. In passing the eye along any of the horizontal lines, dividing the cycle of the engine movements, every cotemporaneous motion, as well as its direction, at that precise time becomes visible; as also the position of those parts which are at rest.

The author then proceeds to state, that he found much time to be saved in the construction of his calculating engine by employing this mechanical notation; and he has, by way of example, in this paper described its application to the hydraulic ram, and to the common eight-day clock, of both of which he has given the drawings and notation.

On the Parallax of the fixed Stars. By J. F. W. Herschel, *Esq. M.A. Sec. R.S. Communicated January* 19, 1826. Read March 9 and 16, 1826. [*Phil. Trans.* 1826, *p.* 266.]

The measurement of the distance between the two stars composing a double star, at opposite seasons of the year, has long ago been suggested as a means of detecting a difference of parallaxes between them, if any exist; but in practice has not proved delicate enough for the purpose, owing to the difficulty with which measures of distance are still attended when minute fractions of a second are to be determined. The author observes that it appears hitherto to have escaped notice that a difference of the parallaxes will affect not only their distance from each other, but also their angle of position, and that this latter effect is much more sensible and measurable for a given amount of parallax than the former; and he accordingly proposes it as a means of determining the question as to the existence or non-existence of a sensible difference, in all stars favourably situated.

He first enters into an estimate of the least amount of such difference which this method is capable of detecting, with our present instruments and methods of observation, which, for two stars at 3″ distance from each other, he states to be one fortieth of a second, and in closer stars a still smaller fraction.

In selecting stars for examination, the most favourable position in

which parallax most affects the angle of position is when the great circle passing through both stars, passes also through the pole of the ecliptic. But to ascertain how far any particular star is or is not favourable to the application of the method, and the times of the year when it ought to be observed, it is necessary to make a calculation, the mathematical principles of which the author explains; and includes, in proper formulæ, which he then applies to the formation of a list of about 70 stars, as a specimen.

He concludes by observing, that by a certain mode of using the double image-position micrometer, first explained to him by Captain Kater, the application of this method ceases to be limited to close stars; and that stars of any moderate distance, otherwise favourably situated, may be equally well subjected to examination with nearer ones; and that thus the range of objects placed within our power becomes unlimited.

A Formula for expressing the Decrement of Human Life. In a Letter addressed to Sir Edward Hyde East, *Bart. M.P. F.R.S. By* Thomas Young, *M.D. For. Sec. R.S. Communicated February* 2, 1826. Read April 19, 1826. [*Phil. Trans.* 1826, *p.* 281.]

The author first observes that an opinion is generally prevalent, of a decided increase in the average duration of human life in many parts of Europe; but he yet regards it as probable that this improvement has been much exaggerated, partly on account of the limited number of persons on whom the observations have been made, and partly from erroneous views respecting the profits of assurance companies.

He then examines the evidence on which this opinion rests, and gives a comparative statement of the annual average of mortality, the mean term of full life, and the mean age of mankind, according to a great number of different authorities; and considers that a prolongation of life to the extent of one year in eight, is a much fairer estimate than one in three, which some have maintained, even on the limited grounds of the experience on which they have reasoned.

Another mode, he observes, of easily appreciating the regularity and analogies of tables is the construction of a curve in which, the abscissa representing the age, the ordinate shall represent the corresponding decrements of life. This he accordingly does, and its inspection he observes is sufficient to render us suspicious of the accuracy of the Carlisle tables; while he considers a combination of these and the Northampton tables, and the London parish registers, as likely to give the fairest estimate.

After commenting on the various documents before him, the author next proposes the formula mentioned in the title, which consists of terms having respectively a preponderating influence in infancy, in youth, in middle age, and in old age.

This is followed by a series of numerical documents, and the values of the formula calculated for each year; and the curve corre-

sponding to this formula is also laid down, and its agreement with the adjusted value placed in evidence. The author concludes with some remarks on an error fallen into by Dr. Price, depending on the periodical payment of interest; and with a comparison of climacteric years, as taken from different tables.

Account of an Experiment on the Elasticity of Ice. By Benjamin Bevan, *Esq. In a Letter to Dr.* Thomas Young, *For. Sec. R.S.* Read April 27, 1826. [*Phil. Trans.* 1826, *p.* 304.]

Mr. Bevan took the opportunity of the severe frost of the last winter to determine the modulus of elasticity of ice, which he did by cutting a rectangular plate of that substance from the surface of a pond of 100 inches in length, 10 in width, and about 4 in thickness. The deflection produced by a weight of 25 lbs. was 0·206 inches, from which he concludes the modulus of elasticity to be 2,100,000 feet.

The modulus for water he states at 2,178,000 feet, on a certain hypothesis respecting its cubical compression.

Results of the Application of Captain Kater's *Floating Collimator to the Astronomical Circle at the Observatory of Trinity College, Dublin; and Remarks relative to those Results. By the Rev.* J. Brinkley, *D.D. F.R.S. P.R.I.A. Communicated by the Board of Longitude February* 2, 1826. Read April 27, 1826. [*Phil. Trans.* 1826, *p.* 307.]

Dr. Brinkley, in this communication, states a number of observations made with the floating collimator of Captain Kater, as applied to the Dublin circle, in which he observes it affords the means of ascertaining the index error with as great precision as by reversion, and that in several points of view it is undoubtedly superior to that method. The reversing principle of the Dublin circle, he observes, serves very conveniently for a measure of the accuracy of the floating collimator, and serves to show very satisfactorily, that applying this instrument to any circle will introduce no error depending on the collimator itself.

The author regards the results of these observations as highly favourable to the principle of the collimator, which he considers as a new astronomical power, and as even belonging to a more advanced era of practical astronomy than the present.

The observations consist of, First, the mean zenith distances of a number of stars, deduced solely by the application of the index correction, as determined by the collimator. In this case the circle was used as a mural circle, or rather as two mural circles, having been used with its face east and also west. Secondly, the inclination of the line of collimation of the collimator, as determined on different days. As this appears to have been very permanent (though such permanence is not essential in practice), he concludes that the collimator is applicable to the most powerful instruments. Thirdly,

the index corrections as determined by the collimator. Fourthly, the index corrections as determined by reversion from several stars.

On the Means of facilitating the Observation of distant Stations in Geodætical Operations. By Lieutenant Thomas Drummond, *of the Royal Engineers. Communicated April* 14, 1826, *by Lieut.-Colonel* H. Colby, *F.R.S.* Read May 4, 1826. [*Phil. Trans.* 1826, *p.* 324.]

In consequence of the Report of a Select Committee of the House of Commons in June 1824, it was resolved that a new survey of Ireland should be undertaken; and the author was directed by Colonel Colby to consider the most effective means of rendering distant stations observable in the prevalent hazy state of that country.

After adverting to some of the usual means resorted to in such cases, and showing their inefficiency for his present purpose, and after noticing some unsatisfactory trials of brilliant pyrotechnical preparations, and of the combustion of phosphorus in oxygen gas, the author attempted applying to the purpose in view the brilliant light emanating from intensely heated quicklime. To obtain the requisite temperature, he passed a stream of oxygen through the flame of alcohol; and this jet being directed upon a small spherical piece of quicklime placed in the focus of a proper reflector, the light which it emitted was found to have 83 times the intensity of the brightest part of the flame of an Argand burner. Other substances, such as zirconia, magnesia, and oxide of zinc, were tried as substitutes, but were found very inferior as sources of light, when ignited, to quicklime from chalk, which moreover admits of being conveniently turned in a lathe into focal balls of any requisite dimensions. Mr. Drummond proceeds to detail the necessary application of this system to a case of considerable difficulty that occurred at the end of last season. Slieve Snaght, the highest hill of Innishowen (2100 feet), and 15 miles North of Londonderry, forms an important point in the triangulation connecting the North of Ireland with the Western Isles of Scotland. On the 23rd of August a conspicuous object was placed upon its summit, that it might be observed from Divvis Hill, near Belfast; but till the 26th of October it remained so enveloped in fog, as to frustrate all efforts at observation, the distance between the stations being 66¼ miles. On the 27th of October the author proceeded to the hill; and after some delay from tempestuous weather, brought the new instrument into use, which was brilliantly visible at the other station, and thus successfully terminated the observations.

This paper concludes with notices of some other proposed applications of these sources of intense light, more especially to certain lighthouses, in which the author thinks that the additional expense would be amply counterbalanced by the advantages of so powerful a source of illumination.

In a note appended to this paper by Mr. Herschel, he observes, that red, yellow, and green, appear to be the predominant rays in

the light emitted by the lime, but that none of the rays are entirely wanting. No black lines crossing the spectrum could be observed. He explains the cause of the peculiar coloured shadows thrown by this light, as compared with those of oil and day-light; and adds, that it is remarkable that the spectrum thus emitted by incandescent quicklime differs from that of the salts of lime, the characteristic colour in the latter case being thick red.

On the Production and Formation of Pearls. By Sir Everard Home, *Bart. V.P.R.S.* Read May 11, 1826. [*Phil. Trans.* 1826, *p.* 338.]

In his examinations of the organs of generation of the large fresh-water muscle, the author often met with seed pearls, either in the ovarium, or connected with the shell upon which the ovarium lay; and he remarked at the same time that all Oriental pearls have a brilliant central cell, which in the common mode of boring them is destroyed, but which may be beautifully exhibited by carefully splitting the pearl into halves: this cell is just large enough to contain an ovum, which is formed upon a pedicle like the yolk of the pullet's egg, and is similarly discharged when completely formed. Thence Sir Everard concludes, that a pearl is formed upon the external surface of an ovum, which having been blighted, does not pass with the others into the oviduct, but remains attached to its pedicle in the ovarium, and in the following season receives a coat of pearl at the same time that the inner surface of the shell receives its annual supply. This conclusion, he observes, is verified by some pearls being spherical while others are pyramidal, in consequence of the pedicle, as well as the ovum, having been enamelled with nacre.

This paper concludes with an extract from one of the early volumes of the Philosophical Transactions, in which a corresponding account of the growth of pearls is announced by Arnoldi in 1673.

On Burrowing and Boring Marine Animals. By Edward Osler, *Esq. Communicated February* 15, 1826, *by* L. W. Dillwyn, *Esq. F.R.S.* Read May 25, 1826. [*Phil. Trans.* 1826, *p.* 342.]

The author's object in this paper is to describe the mechanism by which the boring and burrowing shell-fish form their habitations, and to explain some parts respecting the burrowing of other marine animals.

After showing that the Nereides bury themselves by the undulating motion which they employ in swimming, aided by the action of their bristly feet, and that the *Arenicola piscatorum* forms its imperfect arenaceous tube by the aid of a viscid secretion which exudes from the anterior half of the animal, he particularly describes the habits of the *Terebella conchilega*, showing that by a glutinous secretion it cements together particles of shells and sand, so as to form a collar, which is regularly and curiously lengthened into a tube; and when this is about an inch in length the animal proceeds to

burrow; and having completely concealed itself, is soon found to have turned within the tube. Mr. Osler describes the manner in which this motion is effected by reference to an annexed drawing.

The Spatangus buries itself chiefly by the action of its bristles, while the long dorsal spines prevent the sand from closing entirely, and preserve a small round hole, by which water is admitted to the animal, and which marks its situation.

The author next describes the burrowing of bivalves. The animal projects the foot into the sand, and then gives a circular motion to the shell, sinking at every stroke till nothing but the extremity of the syphon can be perceived above the sand. This motion is effected by two appropriate pairs of muscles arising from the shell, and inserted into the foot, the position and actions of which are explained by reference to a drawing. The locomotive bivalves travel by a series of motions similar to those of burrowing, as seen in the *Venus gallina*, the *Anodonta cygnæa*, &c. The *Buccinum undatum*, though not habitually residing under the sand, is so often observed to bury itself as to come within the author's arrangement: as in the bivalves, its foot is the instrument of penetration, and, like them, it can distend that organ to nearly the size of the shell; the author describes its anatomy by the help of annexed drawings. He also enters into a detailed account of the peculiarities of structure of the Pholas, which has two methods of boring. The first is almost exclusively employed by the young animal; it fixes itself by the foot, and having raised itself almost perpendicularly, executes a succession of rotatory motions; but when the animal is older, the foot being attached as before, it brings the anterior points of the shell so as to press its operative part against the bottom of the hole, and then the dorsal margins of the valves are brought into contact, so that the rasp-like portions scrape over the substance on which they press. In the Teredo, the peculiarities of structure observed in the Pholas exist in an equally marked degree.

Mr. Osler next describes four species of Lithophagi, of which the *Saxicava rugosa* is so abundant at Swansea, as to induce him to take it as the type of the family. Its general structure resembles that of the burrowing bivalves, but it does not admit of boring by any rotatory motion; and several other facts show that the shell is not the instrument by which it penetrates hard substances, such, for instance, as the absence of any arrangement of muscles for the purpose, and it being able to penetrate certain substances only, all of which are of a calcareous nature. Hence the author concludes, that the perforating power of the Lithophagi is referrible to chemical rather than to mechanical causes; and he refers it to some peculiar secretion which the animal forms, and which readily destroys carbonate of lime, whilst siliceous and argillaceous stones resist its action. In conclusion Mr. Osler remarks, that he has made many experiments with a view to detect the nature of the solvent: he shows that it is not of an acid nature: nor has he been able to detect any peculiar soluble compound of lime in portions of water, in which many Saxicavæ had been con-

fined, together with a portion of calcareous rock; yet though such direct proofs are wanting, he considers the collateral evidence as quite satisfactory in proving that a true solvent power over calcareous matter is exerted by those animals.

An Account of some Experiments relative to the Passage of radiant Heat through glass Screens. By the Rev. Baden Powell, *M.A. F.R.S. of Oriel College, Oxford. Communicated March* 9, 1826. Read June 1, 1826. [*Phil. Trans.* 1826, *p.* 372.]

The object of this paper is to examine the correctness and the consequences of a conclusion of De la Roche, that if radiant heat be intercepted by two transparent screens, the additional diminution of effect occasioned by the second is proportionally much less than that produced by the first, and so for any number of screens. This M. De la Roche explains by supposing the heat passing through the first screens to acquire thereby a kind of polarization, which enables it to pass the rest more easily. The author, however, observes, that when the temperature of the source of heat is above incandescence, the fact admits of more simple explanation, by regarding the heat as consisting of two portions, the one incapable of penetrating glass, and therefore wholly stopped by the first screen; the other capable of so doing, and therefore readily transmitted through any number of screens.

If, however, he observes, the same effect is produced at temperatures below luminosity, this explanation fails; and either De la Roche's idea of polarization, or some other, must be resorted to; and it is to the examination of this case that the experiments detailed in his paper are devoted.

His apparatus consisted of two tin reflectors, opposed to each other, and having in the focus of one a blackened thermometer, and in that of the other an iron ball heated to redness, and cooled till it ceased to be visible in the dark. The indications of the thermometer were observed; first, for the direct effect; secondly, with one glass screen interposed; thirdly, with two;—sometimes a mercurial, sometimes an air thermometer was used. The indications were noted after each half minute of exposure, till the thermometer ceased to rise. The temperatures acquired by the screens were also noted. The conclusions drawn by the author from a great number of such experiments are,—

First, That the fact observed by M. De la Roche is verified when the source of heat is below luminosity, as well as above it. For in all the trials a rise was observed to take place in the focal thermometer, much smaller with one than with no screen, and very small indeed with two. The diminution, however, occasioned by the second screen was proportionally much less than that occasioned by the first.

On analysing more minutely, however, the progress of the rise of the temperature from half minute to half minute, and comparing it, in the cases where screens were used, with the observed progressive

increase of temperature of the screens themselves, Mr. Powell is led to conclude that the rise of the focal thermometer, where screens are used, is not attributable to any new property acquired by the heat in its passage through the first screen, when two are used, or to any direct radiation through the glass where one only is employed, but is simply the effect of secondary radiation from the heated screens; and that this cause must have operated extensively, is evident from the circumstance, that the reflectors were placed in some of the experiments at 15 inches from each other, in others only at 12. In the latter case, the first screen was found to have acquired in some cases as much as 23° (centigrade) of temperature above that of the ambient air, its distance from the heated ball being 2 inches.

The author next proceeds to examine the interception of heat by *glass of extreme thinness*; in which case, according to Mr. Ritchie's experiments, heat from non-luminous sources appears capable of radiating through that medium when transparent, but not when rendered opake. His experiments were made with fragments of a large glass bulb blown to extreme tenuity, and either left transparent or blackened with soot; but their results proved unfavourable to Mr. Ritchie's conclusion, no difference having been observed between the effects of thin and thick glass sufficient to warrant any difference in their mode of transmission.

The Bakerian Lecture. On the Relations of Electrical and Chemical Changes. By Sir Humphry Davy, *Bart. P.R.S.* Read June 8, 1826. [*Phil. Trans.* 1826, *p.* 383.]

The author prefaces the experimental results and investigations in this lecture with a brief historical statement of the origin and progress of electro-chemical science, with a view to correct the erroneous statements which have appeared in this country and abroad. In this the first origin of this branch of knowledge is stated to be the discovery of the decomposition of water by the voltaic pile by Messrs. Nicholson and Carlisle in 1800. This was followed by the experiments of Cruickshank and of Dr. Henry, and by several papers by the author himself, the chief contents of which are stated, and in which the appearance of acids, oxygen, and azote at the positive, and of alkalies, sulphur, and metals, at the negative pole, is shown.

The experiments of Hisinger and Berzelius in 1804 are placed next in order, which establish similar results; and in 1806, on the occasion of the agitation of the question respecting the formation of muriatic acid and fixed alkali from pure water, the author presented to this Society his Bakerian Lecture on the chemical agencies of electricity, in which he drew the general conclusion, that the combinations and decompositions by electricity were referrible to the law of electrical attractions and repulsions,—a theory in which, he observes, he has hitherto found nothing to alter, and which, after a lapse of twenty years, has continued, as it was in the beginning, the guide and foundation of all his researches.

The instruments used in the experiments of this paper for detecting and estimating electric currents of small intensity, were constructed on the principles of the multiplier of Schweigger, and the galvanometer of Prof. Cumming; the suspended needle being surrounded, in the direction of its length, with several coils of silked wire, and having its directive force counteracted by two other needles exterior to the coils, being a modification of the method employed for a similar purpose by M. Nobili. For determining weak electricities of tension, Volta's condenser, connected with Bennet's electrometer, was employed; or with one consisting of a silk filament, rendered conducting by charcoal dust. Much dependence was, however, never placed on these instruments, unless their indications were otherwise confirmed.

The author now proceeds to the experimental inquiries, which form the chief object of the lecture, and to the general views of electro-chemical agency to which they appear to lead. First, he considers the electrical and chemical effects exhibited by combinations of one metal and one fluid.

The nature of these effects is best explained by an example. When two pieces of polished copper, each connected with the extremity of the wire of the multiplier, are plunged into a solution of an alkaline hydro-sulphuret, if introduced at the same instant, there is no action; but if in succession, there is a distinct or even a violent electrical effect, and the piece of metal first introduced is negative with respect to the other. This effect depends on the formation of a coat of sulphuret of copper on the plate first introduced, which is negative with respect to metallic copper. Hence the combination is, in strictness, one of these elements,—copper, sulphuret of copper, and the solution.

In like manner, protoxide of copper is negative with respect both to pure copper and to the sulphuret. Hence, wherever instead of pure copper, tarnished (i. e. oxidated), copper is used in the foregoing experiment, the effect will be precisely the reverse; and by this many curious anomalies, in this and similar experiments, are explained.

This production of electrical currents by single metals and single fluids occurs generally whenever new products adhering to the metallic surfaces are produced; and if the same products be applied artificially, the effects are the same as if their adhesion had been caused by the natural action of the fluid on the metal. The chemical changes produced in the fluid by the ternary combinations thus formed, are in all cases such as tend to restore the deranged equilibrium, hydrogen passing to the negative side, and oxygen to the positive, until the oxides are revived.

We come next to consider the case of two imperfect and one perfect conductor, as two fluids and a metal, or charcoal. And here the author takes occasion to combat an opinion advanced on high authority * respecting the active development of electricity in the combinations of acids and alkalies, which he refers to the contact of me-

* That of M. Becquerel.

tals with these agents, to change of temperature, to evaporation, &c., and never to the mere union of the elements; and supports his opinion by several experiments.

When platinum is brought in contact with an acid, the pole touching the acid is negative, the opposite pole positive; and *vice versâ* when it touches an alkali; and the same is the case with rhodium, iridium, and gold: and the same holds good in all cases, the effect being greater as the action of the acid on the metal is greater. From this it follows, that when a metal is in contact with an acid or an alkali in one cup, and water or a neutro-saline solution in another, on completing the circuit, the contact of the metal with the acid or alkali will determine the character of the pole in contact with it; and that in contact with the other fluid will, of course, be of the opposite name, and this result is confirmed by experiment. In such combinations the chemical changes are such as might be expected, oxygen and acids tending to circulate towards the negative surface, and hydrogen and the alkalies towards the positive.

In combinations consisting of two perfect conductors and one fluid, the order in which the metals exhibit their electricities is connected with their oxidability, the more oxidable metal being positive with respect to all below it. It is not, however, any inherent quality in the metals which determines this effect, but their fitness for chemical action; for if the state of aggregation be altered, and the cohesive force, which always acts as antagonist force to chemical changes, be weakened, the positive energy is exalted in proportion: thus the amalgams of the positive metals are positive with respect to the pure metals of which they are amalgams.

In general the electricities developed by metallic contact are too strong to be subverted by an opposite action of the fluids, with which both are in contact. Such, however, is sometimes the case; and in all cases the influence of the fluid is perceptible. An instance is given in the case of zinc and platinum, in contact with each other, and the one immersed in alkali, the other in acid: the energy of electrical action will here be found very much greater when the platinum is plunged into the acid, than in the contrary arrangement. And in this and similar cases, the general law, that the chemical changes produced are such as tend to restore the equilibrium, holds good.

The author next considers the accumulation of electricity, and the chemical changes it produces in voltaic arrangement. According to Volta's view of the action of the pile, the metals were regarded as the only agents, and the chemical changes arising in the fluids as mere results not essential to the development of the electricity. This view, however, is sufficiently opposed by the inactivity of combinations in which no chemical changes occur, but may be regarded as altogether disproved by an experiment here described, in which, when two glasses filled with solution of nitrate of potash, in which were plunged respectively zinc and platina, connected by the multiplier, were connected by substances capable of conducting electricity,

but not of propagating chemical action, such as unoxidable metals, the circulation of the current was altogether destroyed.

Since the chemical changes always tend to restore the equilibrium destroyed by the contact of the metals in the fluids of a pile, it is evident that the relation between the fluids themselves and the surfaces with which they are in contact, will be altered by a continuance of the action of the pile. Hence it is easy to perceive the possibility of a re-action taking place, when the circuit is broken, or the disposition of the parts of a pile is changed, or one or more parts of a compound circuit abstracted. Many curious phenomena, of which hitherto no explanation has been offered, may be explained by this view of the subject; such as the secondary piles of M. Ritter; the supposed polarization of electricity concluded by M. De la Rive from his experiments of the interposition of metallic plates in the fluids of a pile; the continuance of electro-motive action of detached portions of a circuit, after the destruction of the circuit itself, in some experiments, &c. This *re-action* is illustrated in the paper before us by an experiment, in which a circuit, primarily inactive, consisting of six arcs of platinum in vessels filled with solution of nitre, was made part of a battery, consisting of fifty pairs of plates, of a combination primarily active. After continuing the circuit some time it was broken, and the platinum arcs detached and formed into a circuit were found to possess independent action, contrary to that of the pile, which had thus rendered them re-active. This singular consequence is pursued yet further in another experiment here stated, in which detached portions of a battery of fifty plates, which had been some time in action, were examined as separate piles, after breaking up the combination. When they had been placed *conformably* in the original battery, their independent action was found to be very much weakened by the re-action thus produced, which in this case opposed their natural effect; whereas, when *unconformably* placed in the original battery, their action, when detached, was found exalted to three or four times its natural intensity.

The author next proceeds to point out some general observations and practical applications which suggest themselves on a view of the foregoing results. The chemical changes in a conducting liquid, he first shows, take place only in the immediate vicinity of the immersed poles, the rest of the liquid affording only a tranquil passage to the electricity. This leads him to consider the motions produced in mercury when interposed in the circuit under an electrified fluid, which he regards as arising from the two electricities acting as transporters of ponderable matters, which assume their own peculiar characters when they reach their point of rest. The lecture concludes with some practical suggestions as to the use of the multiplier to obtain exact numerical measures of the electro-dynamic relations of chemical elements, and with some applications of the preceding results to the useful arts, especially in the preservation of the copper on ships, and the iron boilers of steam-engines.

On the Discordances between the Sun's observed and computed Right Ascensions, as determined at the Blackman-street Observatory, in the Years 1821 *and* 1822; *with Experiments to show that they did not originate in instrumental Derangement. Also a Description of the seven-feet Transit with which the Observations were procured, and upon which the Experiments were made. By* James South, *Esq. F.R.S. Communicated June* 1, 1826. Read June 8, 1826. [*Phil. Trans.* 1826, *p.* 423.]

Astronomers have generally admitted that a certain discordance between the observed transits of the sun and its computed right ascension takes place, and have been accustomed to refer it to instrumental error, arising from the action of the solar rays on the parts of the transit instrument used.

The validity of this explanation, and the true state of the facts, are what the author proposes to investigate in this communication.

He commences his paper with a full description of the transit instrument used in the observations, (the work of Mr. Troughton,) and of which, as accurate drawings have already been exhibited to the Society, no account need now be given, further than to mention the connexion of the cones which form the axis and the tube by tension bars, acting in their interior by means of differential screws, and thus drawing the cones in forcible contact with the spherical shell, which forms the centre of the instrument.

This instrument was erected in 1820, and its adjustments being made, it was subjected for two months to every species of trial to ascertain their permanence, which proving satisfactory, the regular series of observations were commenced.

These observations are stated at length in the tables attached to this paper and forming part of it. The right ascensions of the sun, deduced from them, are compared with those deduced from the computations in the Nautical Almanac reduced to the meridian of Blackman-street; and at each observation, the state of defence from, or exposure to the sun's rays, of the instrument is stated. This was purposely varied as much as possible, and in many instances the sun was purposely allowed to shine for a full hour on the brace covered with black cloth, to produce as great a difference of temperature as possible in them, which was estimated by an attached thermometer, and in some cases amounted to 16°. Yet no appreciable error in the times of transit was found to arise from this severe trial, especially when the means of several days' observations were taken, whether the computed places of the sun by the Nautical Almanac were used, or the corresponding observed transits of the pole star were taken as zero point.

But the general result of the whole series of observations is, that the sun's observed right ascension is always in excess of his computed.

This deviation goes in many cases to the extent of a whole second of time. The mean excess is from 0″·6 to 0″·7; and it appears from the tables here given to be subject to periodical increase and diminu-

tion, its maximum occurring in June and July, and its minimum in December and January.

But that such a result might not rest merely on the observations with one instrument, the author has collected observations made on corresponding days at the observatories of Greenwich, Dublin, and Paris, all which, without exception, give results very nearly agreeing with those of his own observations; and he finally concludes, that as they cannot possibly arise from instrumental error, or error of observation, he hopes to be able ere long to prove, to the satisfaction of the Society, that their cause is imperfection in the solar tables.

On the Existence of a Limit to Vaporization. By M. Faraday, *F.R.S. Corresponding Member of the Royal Academy of Sciences at Paris, &c. &c. Communicated May* 26, 1826. Read June 15, 1826. [*Phil. Trans.* 1826, *p.* 484.]

The object of this paper is stated by its author to be, to show that a limit exists to the production of vapour of any tension by bodies placed in vacuo, or in elastic media, beneath which limit they are perfectly fixed. The train of argument by which this is attempted to be demonstrated may be summarily stated as follows.

Assuming it as proved by Dr. Wollaston, that a limit exists to the earth's atmosphere, where in consequence of its rarity its molecules are so distant from each other, that the repulsion of any one on the molecule below it is just equal to its gravity, it is clear that in this case the force of gravity may be regarded as setting a limit to further expansion; and if we could exhaust a receiver to the degree of tension obtaining on the surface of the atmosphere, any further subtraction of air would produce no further diminution of density, but would merely produce a vacuum in the upper part of the receiver.

But cold, as well as rarefaction, diminishes the elasticity of vapours or gases, and therefore if the temperature be greatly diminished, the limit above alluded to, where gravity counteracts the elastic force, will be attained with a less degree of rarefaction; and if the temperature were sufficiently low, it is evident that air of any given degree of density would lose its disposition to expand in a direction contrary to gravity.

In the case of air, however, the cold required to render it inelastic at any sensible density would of course be excessive. But if we consider the vapours of very fixed bodies (as silver for instance), whose tension even at a white heat is insensible, it is almost certain that the ordinary temperature of the atmosphere is, with respect to that capable of maintaining it at a sensible tension, such a degree of cold as would effectually bring it under the command of gravity. Supposing then silver to cool from fusion, that moment when these forces became equal, would be the one in which vapour could exist *above* the silver; and at every lower temperature the metal would be perfectly fixed. But the author regards it as probable that this equilibrium at ordinary temperatures may take place with bodies

much more volatile than silver, and states an experiment made with mercury in the winter of 1824-25, where no action on gold-leaf, suspended over it, however near, took place, from which he concludes the mercury then to have been perfectly fixed; and other experiments on mercury and on sulphuric acid by Sir H. Davy and Signor Bellani are adduced in support of the same view.

But there is another force, that of homogeneous attraction, which the author regards as sufficient to overcome a certain degree of vaporous elasticity; and he illustrates the mode of action of this force by an experiment on the slow crystallization of camphor, and by that of other substances from vapour in the process of sublimation; and by analogous phenomena in the crystallization of salts from aqueous solutions.

On Electrical and Magnetic Rotations. By Charles Babbage, *Esq. F.R.S. &c. &c. Communicated May* 29, 1826. Read June 15, 1826. [*Phil. Trans.* 1826, *p.* 494.]

The author first recapitulates the manner in which he conceives time to influence the results of the magnetic phenomena observed by M. Arago, and which need not here be repeated, being in substance that given in a paper on the subject, in the Transactions of last year, stated in a more geometrical form. As the reasoning in this argument requires only that an attractive or repulsive force should be communicated from one body to another in a finite time, it occurred to him that electricity might be substituted for magnetism, and that rotations analogous to those observed by M. Arago might be produced by the use of electrified instead of magnetic bodies. He accordingly suspended by a fine silk thread a thin brass bar with circular ends over a disc of glass; and the bar being electrified by contact with excited sealing-wax, the glass was made to revolve slowly, when the bar was observed to be dragged round in the same direction. The effect was decided; and all proper precautions were taken to avoid disturbing causes, such as currents of air, twist of the silk, &c. The effect was greatest with a slow velocity of rotation, about five turns in a minute. On substituting a stick of excited sealing-wax for the brass bar, the same effect was produced; but when the rotation of the plate was rapid, the stick remained nearly immoveable. The same effect was produced when the glass plate was covered with plates of copper, lead, or other metals cemented to it.

A proper apparatus being constructed for the purpose of further experiments, an excited electrophorus was made to revolve under a flat needle of thin brass with circular ends, with various degrees of rapidity. The motions of the needles were irregular and complicated, but appear to the author capable of explanation, as well as the others on the same principle,—that electricity excited by induction is not instantly destroyed by removing the inducing body.

The great velocity with which electricity travels in conducting bodies having been urged as an objection to this explanation, the author combats this objection, on the ground that small differences of electricity ought (from analogy with a fluid similarly circumstanced,) to be equalized more slowly than great ones; and moreover, that by the disposition of the apparatus, extremely weak forces being made to act constantly and for a long time, ought to produce an effect much superior to that arising from the transient action of each part.

The author next proceeds to examine the action of screens interposed; and having first satisfied himself by direct experiments with unelectrified metallic plates and needles, that currents of air driven through a screen of muslin produced no sensible effects, he placed a screen of coarse gauze between an electrified sealing-wax needle and a revolving pewter plate, and to his surprise found a tendency to motion in the needle, opposite to that of the plate; and though not always produced, nor always to the same extent, this effect occurred in seventeen out of twenty trials.

The author then looked out for some method of increasing the inequality of distribution of electricity on the metallic plate; he therefore placed on the revolving apparatus, just under the metal plate, a very small lighted lamp. Several experiments made with this disposition of the apparatus are related, without leading to any conclusion as to the action exerted on the needle; and the author then proceeds to consider what extraneous causes could have acted to produce the small retrograde motions observed. First, such causes are enumerated as currents of air in the room, in the box containing the apparatus; currents driven through the screen by rotation; currents of heated air from the lamp; vibrations from the mechanism producing the motion; torsion of the wire *suspending*, or electricity of the bridge *carrying*, the needle; and flexure of the wax.

Before relating the experiments made to elucidate each of these disturbing causes, however, he proceeds to relate other experiments, confirmatory of the fact of the retrograde motion, and of the influence of the heat in producing it; after which, he describes a great variety of experiments, made for the purpose of trying the effect of the presence or absence of the disturbing causes; and concludes that none of them, singly or combined, are adequate to produce the phenomena observed.

He then proceeds to state what appears to him to be their true explanation, on the principles adopted respecting the non-instantaneous communication of electricity; or at least to show that they are not repugnant to those principles; or that, moreover, those principles may in certain cases give rise to a retrograde motion, or to no motion at all, or a direct one, according to the disposition of the apparatus; and that very trifling apparent differences in the latter respect may give rise to all the varieties of the phenomena.

Case of a Lady born blind, who received Sight at an advanced Age by the Formation of an artificial Pupil. By James Wardrop, *Esq. F.R.S. Edinb. Surgeon Extraordinary to the King, &c. Communicated by the President.* Read June 15, 1826. [*Phil. Trans.* 1826, Part III. *p.* 529.]

The lady, the subject of this communication, shortly after her birth, was perceived to be blind by a peculiar groping manner, and an operation was therefore, at six months of her age, performed on both her eyes; in consequence of which, she lost the whole globe of one, and the pupil of the other became closed. Her blindness, up to the forty-sixth year of her age, was so complete, that she could barely distinguish in sunshine, or in brightest moonlight, which way the light came. This, however, being sufficient to indicate a sound state of the nerve, three operations were successively performed on her eye by the author, for removing a portion of the closed iris. They were attended with but very slight inflammation, and proved successful in imparting vision. A journal of her progress in the art of seeing, and of her sensations, as expressed in her own words, from the first to the forty-second day after the last operation, is given in the paper before us. She appears, up to that period, to have acquired but very imperfectly, if at all, the power of directing her eye to any given object; catching it only by repeated trials, and as it were searching for it; nor was she at that time yet capable of rightly appreciating the distances of objects. Colours, however, forms, and relative situations, in angular positions, were distinguished much earlier; the former immediately, the latter after very short practice. She appeared equally delighted and bewildered by her new sense; pleased with gay colours and sparkling objects, but most profoundly and permanently affected by the grand features of nature,—the clear blue sky, the fields and trees.

The author regards this case as instructive in many points, especially in a physiological one, as showing the possibility of preserving nervous sensibility unimpaired, during so very long a period of complete inaction.

On the progressive Compression of Water by high Degrees of Force, with some Trials of its Effects on other Fluids. By J. Perkins. *Communicated by* W. H. Wollaston, *M.D. V.P.R.S. May* 25. Read June 15, 1826. [*Phil. Trans.* 1826, Part III. *p.* 541.]

Mr. Perkins first describes the machine which he employed in his experiments. It consisted of a cylinder of gun-metal, 34 inches long and $13\frac{1}{2}$ external diameter, having an internal cavity 29 inches long and $1\frac{1}{2}$ inch diameter; into the upper part of which is screwed a steel pump, $8\frac{1}{2}$ inches long and $1\frac{1}{4}$ inch external diameter, and $\frac{4}{10}$ths externally. The pump has a valve opening inwards at its lower extremity, and a conical enlargement at the top, and the piston is rendered water-tight without stuffing. A lever apparatus is properly

annexed, for the purpose of measuring pressure; and is so adjusted, that the number of pounds pressing on its piston indicates directly the number of atmospheres used for compression. The author next describes the piezometer which he employed in measuring the compression valve, consisting of a tube of water inserted in quicksilver, the contracted part of which tube contained a small steel disk, having a hair spring attached to it to keep it in its position when pressed up. The piezometer, properly arranged, was then placed in the receiver of the compressor, filled with water at 50°, the pump screwed into its place; and as soon as the intended pressure had been effected, the piezometer was examined, and the indicating spring was always found more or less raised in the tube, according to the power employed. Mr. Perkins states that water, under a pressure of 2000 atmospheres, compressed in a tube eight inches long, was diminished one twelfth of its length; and has annexed to his paper a table, showing in inches and parts the compression of a column of 190 inches of water, corresponding to every 10 atmospheres to 1000 inclusive.

The author found that acetic acid crystallized under a pressure of 1100 atmospheres; that under a pressure of 500 atmospheres water took up its volume (?) of air, none of which was again given out on removing the pressure; that air, under a pressure of 1200 atmospheres, became a limpid liquid (permanent?); and that carburetted hydrogen was entirely liquefied under the same pressure.

On the Figure of the Earth. By George Biddell Airy, *M.A. Fellow of Trinity College, Cambridge. Communicated by* J. F. W. Herschel, *Esq. Sec. R.S.* Read June 15, 1826. [*Phil. Trans.* 1826, Part III. *p.* 548.]

Account of Experiments made with an Invariable Pendulum at the Royal Observatory at Greenwich, and at Port Bowen, on the eastern side of Prince Regent's Inlet. By Lieutenant Henry Foster, *R.N. F.R.S.* Read April 6, 1826. [*Phil. Trans.* 1826, Part IV. *p.* 1.]

The author, in the prefatory introduction to this paper, states first of all his own previous experiments made by Captain Hall and himself, on the South American station; he then describes minutely the nature of the experiments contained in this communication; the instruments employed; the precautions used; and the method pursued in calculating their results. The experiments comprise three distinct series. The first made at the Royal Observatory at Greenwich, before his departure with Captain Parry for the North Western Expedition, in which it will suffice to remark, that all the adjustments and precautions recommended by Captain Kater in his paper of 1819 were strictly adhered to. In addition to which, observations were made, not only of the disappearances of one pendulum behind the other, but also of its reappearances, as recommended by Captain Sabine; methods which, he observes, as far as the deduction of the acceleration of the pendulum when compared at different stations

is concerned, give results perfectly identical, as he shows by actual comparison of his own final numbers.

The second series of experiments were made at Port Bowen, where the ships of the North Western Expedition wintered from 1824 to 1825. The observatory was erected near the harbour, 100 feet above the sea, on secondary limestone, on a hard frozen soil, and the locality is very minutely described. The apartment in which the observations were conducted was thickly lined with fearnaught cloth. At first it was attempted to warm it by a stove; but the fluctuations of temperature so produced proved too great, and the stove was therefore removed outside, and the observatory warmed by the smoke-pipe; while the whole apparatus was fenced from draughts of air and sudden change of temperature by a large envelope of fearnaught lined with racoon skins. These precautions proved so effectual, that the total change of temperature during the observation was seldom more than 3°, and frequently not 1°, from 50°; while by a Six's self-registering thermometer, the mean range of temperature, in 24 hours, to which the pendulum was exposed, was only 8°, and the extreme 12°; while that of the atmosphere varied from 23° to 47° irregularly.

During the whole of these observations, every precaution was used to secure and examine the stability of the whole apparatus, and that with perfect success. The time was determined by transits of the sun, Arcturus, and α Lyræ. The clock was one belonging to the Royal Society, fitted with a gridiron pendulum, suspended on knife edges. The transit instrument was of 30 inches focus and 2 inches aperture, cemented by plaster of Paris to a large stone placed on a cask full of sand.

A second distinct series of observations, under very favourable circumstances of weather, was made in July, the results of which differ only one tenth of a vibration in 24 hours from those in June; and a mean of both, according to the number of factors in each series, gives the number of vibrations for Port Bowen.

The third experiment was made at Greenwich, on the return of the Expedition in November, 1825. The number of vibrations in 24 hours derived from it, differed 0·24 of a vibration from that concluded from the first experiment, and this difference was maintained on repetition. The author attributes this to wear of the knife edges, a fine line of metal being visible on the agate planes supporting them. Supposing this wear uniform, the author takes the mean of the two determinations to compare with that at Port Bowen.

He concludes his introduction by a justly merited acknowledgement of the efficient cooperation afforded by the commander of the Expedition throughout the whole of these researches.

The remainder of this communication consists of an ample and very minute and regular detail of the series of observations. They commence with the first experiments at Greenwich, which continued from the 20th to the 25th of April, 1824. The time here has been deduced by comparison of the pendulum clock with that of the transit room of the Observatory. A table is given of these comparisons, regu-

larly made before and after each observation of coincidences, and is followed by a table of the deduced rates of the former clock.

Tables are next given of the observed coincidences, noting the times of the disappearance and reappearance, and their mean for each of ten coincidences, the arcs of vibration, the mean arc, the intervals in seconds, and the corrections for the arcs of vibration; and summed up at the end of each observation, so as to give the mean intervals; the number of vibrations in 24 hours, as observed and as corrected for the arc; the state of the barometer and thermometers is also given at the beginning and end of each observation, which were continued twice a day to the 25th. The whole of the observations are then summed up in one table, and these are then reduced for the rate of the clock, and presented together in a table of results; after taking the mean of which, the proper corrections for buoyancy and elevation above the level of the sea are applied to the result.

The same system of registering is followed in the experiments at Port Bowen; only that here the observations of the transits by which the rate of the clock is determined are prefixed, and the hygrometric state of the atmosphere is also quoted in each observation of coincidences. The observations of coincidences set down in the first series extend from June 14th to June 23rd, and were repeated four times a day. The rates of the clock, as deduced separately from the sun and stars, are tabulated, and the results separately computed for each. In each coincidence, the number of transit observations on which the time depends, multiplied by the interval in days between them, is regarded as a factor, and the sum of these factors is taken to express the weight of the mean determination. The mean of these determinations, according to their weight, is then taken, and the corrections applied as usual for buoyancy, and for geological and local situation.

The second series at Port Bowen extends from July 6 to July 10; but the arrangement of the observations being in all respects similar to that just stated, need not be repeated.

The third experiment at Greenwich is stated in a manner precisely similar to the first, and continued four days, each of ten coincidences being observed twice a day.

The general results may be summed up as follows:—

At Greenwich, the number of vibrations *per diem*, all corrections made, = 86159·368; at Port Bowen, = 86230·172.

The latitude of Greenwich is well known. The observations for that of Port Bowen are not given here, but that element is assumed from observations stated in the Appendix to Captain Parry's third voyage.

Computing on it, the author obtains an ellipticity of [illegible], and an equatorial pendulum of 39·009805 inches. These are by the method of disappearances, the difference between these and the results obtained by reappearances is insensible.

Observations on the diurnal Variation of the Magnetic Needle, at the Whale Fish Islands, Davis's Strait. By Lieutenant Henry Foster, *R.N. F.R.S.* Read April 13, 1826. [*Phil. Trans.* 1826, Part IV. *p.* 71.]

Magnetical Observations at Port Bowen, &c. A.D. 1824-25, *comprehending Observations on the diurnal Variation and diurnal Intensity of the Horizontal Needle; also on the Dip of the Magnetic Needle at Woolwich, and at different Stations, within the arctic Circle. By Captain* W. E. Parry, *R.N. F.R.S. and Lieutenant* Henry Foster, *R.N. F.R.S.* Read April 13, 1826. [*Phil. Trans.* 1826, Part IV. *p.* 73.]

Abstract of the daily Variation of the Magnetic Needle No. 2. *By Lieutenant* Henry Foster, *R.N. F.R.S.* Read April 13, 1826. [*Phil. Trans.* 1826, Part IV. *p.* 118.]

Observations for determining the Dip of the Magnetic Needle. By Captain W. E. Parry, *R.N. F.R.S. and Lieutenant* Henry Foster, *R.N. F.R.S.* Read April 13, 1826. [*Phil. Trans.* 1826, Part IV. *p.* 126.]

Observations on the diurnal Changes in the Position of the Horizontal Needle, under a reduced directive Power, at Port Bowen, 1825. *By Lieutenant* Henry Foster, *R.N. F.R.S. Communicated January* 12, 1826. Read April 13, 1826. [*Phil. Trans.* 1826, Part IV. *p.* 129.]

A Comparison of the diurnal Changes of Intensity in the Dipping and Horizontal Needles, at Port Bowen. By Lieutenant Henry Foster, *R.N. F.R.S. Communicated February* 25, 1826. Read April 13, 1826. [*Phil. Trans.* 1826, Part IV. *p.* 177.]

Account of the Repetition of Mr. Christie's *Experiments on the Magnetic Properties imparted to an Iron Plate by Rotation, at Port Bowen, in May and June,* 1825. *By Lieutenant* Henry Foster, *R.N. F.R.S.; together with Mr.* Christie's *Remarks thereon.* Read April 13, 1826. [*Phil. Trans.* 1826, Part IV. *p.* 188.]

In these communications are recorded all the magnetic observations made by Lieutenant Foster, alone or in conjunction with Captain Parry and the other officers of the North Western Expedition, in the years 1824 and 1825; and they embrace a variety of points of prominent interest in the theory of magnetism. They are digested under separate heads, according as they refer to one or other of the following points:—

1. The variation, and its daily and hourly change.
2. The dip, and the changes observed in it.

3. The intensity of the earth's magnetic force, as estimated in a horizontal plane, and that of the dipping-needle, as referred to its natural direction.

The variation of the magnetic needle is the deviation of the direction in which it rests from the astronomical meridian, or the angle between the planes of the magnetic and astronomical meridians. This angle has long been known to be in a constant state of change, and its alterations have been observed not only from year to year, but from day to day. It has been ascertained that, independent of the gradually progressive change by which the magnetic meridian shifts its direction through large arcs in long times, a daily oscillation takes place, which, in these magnetic latitudes, is of small amount, and can only be rendered prominent by neutralizing the principal part of the earth's directive power, according to a method proposed and practised by Mr. Barlow. In the high magnetic latitudes visited by the Expedition, however, the horizontal directive force of the earth is naturally so much weakened by the effect of the dip, as to allow these oscillations to be observed with great distinctness, without artificial aid, by merely suspending the needle by a silk fibre. By this mode of observing, Captain Parry and Lieutenant Foster have found the diurnal change of variation to be seldom less than one degree, and sometimes to have amounted to five or even seven degrees; with this remarkable addition, that the changes in its amount appeared to them to have obvious reference to the position of the sun and, less distinctly, to that of the moon. They decline, however, entering into any investigation of the laws of the influence of these bodies, leaving them professedly to those who are theoretically conversant with these subjects.

In casting our eyes down the table of variations, in which are registered, hourly and frequently half-hourly, from the beginning of December, 1824, to the end of May, 1825, the positions assumed by two needles (whose constructions, &c. are minutely described),—it is impossible not to be struck with the unsteadiness of the needles. They appear to have been in a perpetual state of fluctuation, advancing or receding alternately and by impulses, and in some instances passing their mean positions from side to side as often as nine times in the twenty-four hours. This irregular fluctuation is one of the most remarkable features of this class of the observations.

By an abstract of this table, in which the positions of the sun and moon, the state of the weather, the aurora borealis, &c. are recorded, it appears that the influence of the sun in increasing the diurnal oscillation is much more marked than that of the moon; and that, contrary to received opinion, the aurora borealis seemed to have had no influence. The regular increase of the amplitude of the diurnal oscillations with the advance of the sun to the north is very striking, and not to be mistaken.

A series of observations on the horizontal position of the needle under a directive force, reduced by Mr. Barlow's method, by Lieut. Foster, forms the subject of another part of this communication.

In the course of these observations, the fluctuating state of the directive energy was frequently rendered remarkably obvious, and in one instance not only daily and hourly, but even momentary oscillations were perceived. This observation induced Lieut. Foster to examine the intensity of the magnetism during those oscillations, in which a corresponding fluctuation was detected, and that so rapid, as to compensate itself during the time of the needle performing 60 vibrations, though its effects were very sensible in intervals of only ten. Accordingly, he was induced to refer this fluctuation in position to a fluctuation in intensity as its cause.

The next branch of this inquiry is directed to the dip of the needle at the various stations visited by the Expedition, and especially at Port Bowen; when the mean dip was found, by a series of observations, extending from November 1 to June 27, to be 88° 1′ 23″.

The intensities came next under consideration. The relative intensities at the different stations are first deduced by vibrating various dipping-needles in the plane of the magnetic meridian. This gives the actual, or, as it may be termed, the natural intensity of the earth's directive force, unreduced by the effect of the dip. But besides this, Lieutenant Foster has instituted a separate series of experiments at Port Bowen, in which the same needle, being alternately suspended as a dipping and a horizontal needle, its times of vibration under both circumstances were observed; and from these observations the author thinks himself entitled to conclude that the changes actually observed in the apparent intensity of the horizontal directive force, are not due to any real and general change in the total magnetism of the earth, but arise only from a minute change in the dip itself. He observes that, without entering into minute calculations on the subject, he believes it will be found that if the magnetic pole of the earth be supposed to describe a small circle about its mean position, of about 2′ or 2½′ in radius, it will reconcile to a considerable degree of precision nearly all the observations of the daily variations, both in direction and intensity of the horizontal magnetism, both in Europe and in the arctic circle.

These communications are terminated by an account of the repetition of Mr. Christie's experiments on the magnetic properties communicated to iron plates by rotation, and with a paper of remarks thereon by Mr. Christie himself. The observations are given in an abstracted form, but accompanied with a statement of them in full detail. In the course of these observations the phenomena observed by Mr. Christie on a minute scale, were here, by reason of the advantageous geographical situation of Port Bowen, so strikingly developed as to excite the greatest interest in all who witnessed them. A perfect correspondence was found between the direction of the deviation due to rotation, as observed and as predicted by Mr. Christie; and a numerical agreement in the results, as great as any expectation could warrant, obtained.

The whole of these results, Mr. Christie observes, prove that the phenomena due to rotation are not merely of theoretical but of practical importance, as connected with the problem of correcting the

deviation of the compass on ship-board by an iron plate; for should circumstances require the removal and replacement of the compensating plate in high northern latitudes, its magnetism might be so altered by the effect of rotation as materially to injure its compensating property. The means of avoiding this disagreeable consequence are pointed out.

Observations to determine the Amount of Atmospherical Refraction at Port Bowen in the Years 1824-25. *By Captain* W. E. Parry, *R.N. F.R.S.*; *Lieutenant* Henry Foster, *R.N. F.R.S.*; *and Lieutenant* J. C. Ross, *R.N. F.L.S.* Read June 15, 1826. [*Phil. Trans.* 1826, Part IV. *p.* 206.]

The author commences by observing, that on attempting the various methods proposed by astronomers for ascertaining by actual observation the amount of atmospherical refraction at low altitudes, they all proved impracticable at Port Bowen, by reason of the intense cold, which rendered it impossible to use the repeating circle or other similar instruments. The method therefore proposed by Lieutenant Foster, and modified by Captain Parry, which was found successful, was, to place a board edgeways and truly horizontal on that part of the high land behind which a given star set, and observe the moments of its disappearance. Then, determining at leisure the zenith distance of the upper edge of the board on the return of the sun, and in weather better fitted for delicate observations, the stars fixed on were α Aquilæ and Arcturus; and the paper before us gives a detailed account of a series of observations of the moments of disappearance of both these stars, and also of the zenith distances of the boards employed by the several observers enumerated in the title. In some cases also, the reappearance of the star below the board was observed, thus giving an observation at another altitude, and the angular breadth of the board was afterwards measured by a micrometer from the station of observation.

Description of a Percussion Shell, to be fired horizontally from a common Gun. By Lieutenant Colonel Miller, *late of the Rifle Brigade, and now unattached. Communicated by* R. I. Murchison, *Esq. F.R.S.* Read November 16 and 23, 1826. [*Phil. Trans.* 1827, *p.* 1.]

In this paper, the author first considers the theory of rifles, with which the subject of it is intimately connected; and regarding it as an admitted principle, that irregularities in the flight of shot arise from irregularities either in their surface or substance, shows how the rotatory motion of a rifle ball, by presenting every part uniformly to the action of the resisting medium, obviates the effect of these irregularities. The spiral or rotatory motion of the ball in rifles, is generally supposed to arise wholly from the re-action of the grooves in the barrel, or from the indentations made by them in the surface of the ball; but the author, taking into consideration the powerful action of the air on projectiles, is led to conclude that the rotation

of a grooved ball may be sustained during its flight, or even produced originally by the resistance of the air acting on the inclined planes formed by the grooves, on the same principle as the rotation of the sails of a windmill. These considerations led the author to conceive the possibility of giving the spiral motion to grooved shot fired from a plain barrel; an idea which, he remarks, seems to have also occurred to Mr. Robins, who left, however, no clue to enable us to discover the nature of his plan. He accordingly commenced a series of experiments for the purpose, and, abandoning all idea of success with spherical shot, adopted the cylindrical form. His first trials were made in the summer of 1821. Hemispherical ends were adapted to cylindrical shot, but abandoned, it being found desirable so to dispose the weight as to give the greatest possible length to the shot; grooves of various dimensions were tried, and were found not to answer when narrow, but required to be wide enough to allow their sides to be exposed, from one end to the other, to a current of air blowing straight between them.

In 1822, some further experiments were made at Woolwich, with grooved leaden shot fired from musket barrels, and with wooden shot from a 5½-inch howitzer, in which the shot, being received on targets, or in banks of earth, were in numerous instances found to have flown in the manner expected, *i. e.* point foremost. Similar trials with grooved leaden bullets from a plain barrel, were made in 1823, to the extent of several hundreds; and the balls, when well made, were always found to fly end foremost. Large wooden grooved shot were also fired from a 24-pounder, at Kinsale Fort, and on some occasions by night with lighted fusees in their sides, by which contrivance it was distinctly seen that the spiral motion was acquired and maintained steadily throughout their flight.

Having thus proved the possibility of communicating the spiral motion to a grooved shot from a plain barrel, it next occurred to the author, that a shell so constructed, and flying always with one end foremost, might be made to explode by percussion. This he accomplished by forming his shell into a cylinder terminated by a conical apex, in which is formed a vent communicating with the cavity of the shell. This vent is plugged with an iron peg, under which is placed a pellet of percussion powder, and which, on the point of the cone striking a hard substance, is driven in and ignites the percussion powder, which immediately communicates with the bursting charge.

Several shells of this construction were fired against Kinsale Fort from a 24-pounder, and exploded on striking it; and the author goes on to describe a great variety of trials with iron shells of different sizes, and fired under different circumstances, both at Kinsale Castle and Leith Fort, in all of which a considerable amount of success was obtained, and in some cases the explosions of the shells took place on striking at 800, 850, and even 1200 yards.

Further experiments were tried at Woolwich in 1826, on the hull of a 28-gun ship, during three successive days. Out of thirty-one shells fired at her, at various ranges from 330 to 450 yards, eleven exploded on striking; one of which took effect on the mainmast and

set it on fire, and the rest did more than usual execution in the hull of the vessel. Others were fired against a bank and target at 800 yards, and when dug out were found, in several instances, point foremost.

The author concludes this paper with some observations on the theory of such shells, and with remarks on their proper shape and proportions, and the practical purposes to which they may be applicable.

On the relative Powers of various metallic Substances as Conductors of Electricity. By Mr. William Snow Harris, *of Plymouth, Surgeon. Communicated by* J. Knowles, *Esq. F.R.S. November* 14, 1826. Read December 14, 1826. [*Phil. Trans.* 1827, *p.* 18.]

The principle on which the author proposes to found a numerical estimate of the conducting powers of metallic bodies, is, that these powers are in some inverse ratio of the heat evolved during the passage of an electric charge through them; and his mode of applying this principle to practice, consisted in the inclosure of wires of the different metallic bodies to be examined, in a given volume of air contained in a glass vessel, and pressing on a column of coloured liquid in a tube of small diameter communicating freely with it. The heat developed in the wire by the discharge of a battery of given surface charged to a given tension, being communicated to the air in the globe, expands it, and raises the liquid in the tube through a space, which, being read off on an attached scale, becomes a measure of the heat.

After describing the precautions used to insure results comparable with each other (such as those employed for obtaining an equal electric discharge in each experiment from a battery of 25 square feet of coated surface, the drawing of all the wires through the same holes to secure this exact equality of diameter, &c.), he proceeds to state the results of an extensive series of experiments. The simple metals tried were copper, silver, gold, zinc, platinum, iron, tin, and lead; and the heats evolved from each were found to be in the order in which they are here set down, that from copper being the least, and from lead greatest, of all the substances tried, being in the proportion of 6 to 72, or 1 to 12. The following are the principal conclusions to which his experiments have led him.

The conducting powers of silver and copper are alike, also those of iron and platinum, and those of zinc and brass. That of lead and tin, he states as being in the ratio of 2 : 1, being an obvious inversion of the ratio, and the same he observes of zinc and gold. Gold to copper, he states as 2 : 3; zinc to copper or silver, as 1 : 3; platinum or iron to copper or silver, as 1 : 5; tin to copper or silver, as 1 : 6; and lead to copper or silver, as 1 : 12.

The conducting powers of metals when alloyed are variously affected. Thus, the conducting power of gold and copper, or gold and silver, when alloyed together, is worse than that of either metal separately, and the difference increases with the quantity of the metal of inferior conducting power present. On the other hand, alloys of

copper and silver, in all proportions, have the same conducting powers with each of those metals separately; while in alloys of tin and lead, the heat evolved is a mean of that of the heats evolved from each metal separately, taking into account the relative weights of each metal present in the alloy.

He observes, further, that very small quantities of alloy influence materially the conducting power. Thus he found that wires, drawn from foreign gold coins, reputed to be very pure, conducted much worse than when drawn from the same gold refined.

The substitution of a flattened for a cylindrical wire, or the separation of the latter into four smaller wires, were found to have no influence on the quantities of heat developed.

On the Expediency of assigning Specific Names to all such Functions of simple Elements as represent definite physical Properties; with the Suggestion of a new Term in Mechanics; illustrated by an Investigation of the Machine moved by recoil, and also by some Observations on the Steam Engine. By Davies Gilbert, *Esq. M.P. V.P.R.S. &c.* Read January 25, 1827. [*Phil. Trans.* 1827, *p.* 25.]

The author commences this paper by stating the necessity of distinguishing by separate appellations all such functions as measure the intensity of physical properties, which he considers rendered obvious by a reference to the controversy respecting motion. The subject of this controversy, he observes, was the measure of motion itself, it being contended on one hand that the motion of a body is always proportional to its weight multiplied by its velocity; this opinion being supported by reference to the properties of the common centre of gravity of systems, &c.; while on the other hand the affections of elastic bodies in collision, and the general law of the conservation of living or active forces, were adduced in support of the latter measure. No sooner, however, were the terms "momentum" and "impetus" introduced into the science of mechanics, than the opinions of the contending parties were reconciled by the removal of every ground of dispute.

In the Bakerian lecture on the force of percussion, read to this Society in 1806, he observes, it is remarked, that neither impetus nor momentum are usually correct measures of the effective action of machines. The criterion of this is the force exerted, multiplied by the space through which it acts, and this measure numerically expressed has been denominated DUTY by Mr. Watt; and the raising of one pound one foot high has been by him made the dynamic unit; according to which estimate, the *duty* performed by one bushel of coals, of 84 pounds, has been found to vary from 30 to 50 millions of such units, according to the nature of the engine, and the mode of combustion. To the measure or function represented by the force applied, multiplied by the space through which it acts, the author, however, proposes to give the name *efficiency*, retaining the word *duty* for a similar function, indicative of the work performed; and by a

comparison of these two functions, viz. the efficiency expended on, and the duty performed by, any machine, an exact measure of its intrinsic work will be obtained.

The author then proceeds to instance the utility of this new term in investigating the mechanical value of the recoil-engine; and by an algebraic process, taking every thing most favourable to the engine, arrives at the conclusion that the duty cannot, even in the best state of its action, materially exceed half the efficiency, and that in consequence it can never be used with advantage, the water-wheel and the pressure-engine offering much greater duties; while the wheel possesses the advantage of preserving a uniformity of efficiency during its whole action, which is not the case with the recoil-engine. And these considerations lead him to remark on the impossibility of carrying into effect a plan proposed by some eminent engineers for applying steam on a principle of recoil.

To estimate the efficiency of steam acting uniformly with its entire force, the author assumes from experience that a bushel of coals can convert into steam 14 cubic feet of water, occupying 1330 times that space in the state of steam, and therefore lifting an atmosphere incumbent on the surface of the water uniformly to 1330 times its depth; thus giving an efficiency of about 39 millions of pounds, raised one foot high. From this he concludes (all deductions made), 30 millions would probably be the utmost attainable limit of duty, but for two expedients; 1st, causing the steam to act expansively, after exerting its whole force through a certain part of the cylinder; 2ndly, raising its temperature, by an increased expense of fuel, much above 212°.

Both these means are considered, and occasion is taken to compare the efficacy of the methods invented by Messrs. Watt and Hornblower for the former purpose, the preference in point of simplicity and advantage being given however to the former. With regard to the latter, it is concluded that in certain cases, advantage is really gained by the use of strong steam. The author then alludes, with approbation, to a method recently attempted, where a small quantity of water is forced at each stroke into a minute boiler; presenting, however, a very large surface, in proportion to its capacity, and kept at an equable high temperature by immersion in fused metal. But he considers the greatest hopes of increased power to rest on the application to mechanical purposes of some fluid more elastic than the vapour of water, according to the suggestion of the President, in the Philosophical Transactions for 1823.

The author concludes this paper by a statement of the duties actually performed by the engines in Cornwall; from which it appears that several of the large engines there at work are actually performing a duty greater than the whole efficiency of the steam, unaided by expansive working or high pressure, on the assumptions here made; while others, apparently similar in every respect, fail of performing half that duty,—and no satisfactory cause has been assigned for that important difference.

The Croonian Lecture for 1826. *By Sir* Everard Home, *Bart. V.P.R.S.* Read November 16, 1826. [*Phil. Trans.* 1827, *p.* 39.]

The author states the subject of this lecture to be, "an inquiry into the mode by which the propagation of the species is carried on in the common oyster, and the large fresh-water muscle."

After noticing the agreement of these animals, in the circumstance of their ova becoming the nuclei on which pearls are formed, he observes that they disagree in the process gone through before the young is completely formed. The mode of propagation of the oyster he regards as more simple than that of many plants; and the processes, as gone through in much shorter time.

In describing the mode of breeding of the oyster, the author first observes that we labour under considerable disadvantage in the inquiry in this country, from the prohibition of their sale during the spawning season; from which, however, he was relieved by a weekly supply of oysters from a private bed.

He first describes the two ovaria, which he states to lie immediately within the membrane that lines the two shells, having the liver between them, whose structure resembles so closely that of the ovarium containing ova, as only to be distinguished by their colour and situation. In March, the ova were large enough to be visible in the field of a microscope, and were spherical; as they enlarge, that membrane to which they are attached becomes thickened. In June, they have attained their full size, and are seen surrounded by a white liquor, which the author regards as impregnating fluid. At the time of their detachment a tube is seen, not to be detected before, originating by an opening between the two ovaria. This is the oviduct; and the embryo, when it enters it, has already acquired a shell. About the end of June, the young begin to leave the ovaria, and at the end of July none are found, either in them or in the oviduct.

The author then observes, that the oyster seems to form an exception to the general rule observed by other fish,—being out of season, and having no flavour during the time that the ovaria are full of ova. In fact, however, the breeding season of oysters, when the ova are getting ready for impregnation, is March or April; while in June or July they may be said to have spawned, the embryo being then received into the mouth of the oviduct.

In the fresh-water muscle the ovaria resemble, in situation and appearance, those of the oyster; and the ovaria are the same size before impregnation, which in them also happens in the ovarium. They are completely formed about the 10th of August, and about the 20th are detected passing into the oviduct; and about the 12th of September have all arrived there. That they are previously impregnated is evident from the ovum having been formed into a vesicle, through which the embryo may be seen opening and shutting its shells. The author here notices a curious phenomenon, many of the young being seen in this state, turning round and round as on a centre. Mr. Bauer ascertained that this motion is produced by a

worm, which gets into the vesicle, and performs these revolutions while feeding on the young muscle, carrying it round with it, though itself invisible.

The young remain in the oviduct, which is like a honey-comb, till they arrive at the size fitting them to provide for themselves; and leave it in October and November; and when ready to leave it a canal is formed, through which they pass out,—an operation facilitated by the motion of the foot of the parent, which is partly surrounded by the oviduct.

On a newly discovered Genus of Serpentiform Fishes. By I. Harwood, *M.D. F.L.S. Professor of Natural History in the Royal Institution of Great Britain. Communicated by* Daniel Moore, *Esq. F.R.S.* Read February 1, 1827. [*Phil. Trans.* 1827, *p.* 49.]

Dr. Harwood begins this paper by observing, that in no department of natural history have descriptions been more unsatisfactory than such as relate to certain productions of the ocean, which, from the immeasurable depths which conceal them, and absence of the circumstances best adapted to their multiplication, very rarely present themselves to our notice. It is to this rarity of opportunities for exact examination that we are to attribute the wonderful accounts of sea-monsters, which have from time to time appeared, such as the Kraken, the Sea Serpent, &c.

The author, after this preface, proceeds to describe a very extraordinary marine animal, taken by Captain Sawyer, of the ship Harmony, which was in pursuit of the bottle-nosed porpoise, in lat. 62° N., and 57° W. long. He observed a body floating on the water, which he at first took for an inflated seal-skin, but on a nearer approach it proved to be a living animal. Exhausted by unavailing efforts to gorge a fish, seven inches in circumference, it allowed itself to be taken, and was preserved by Captain Sawyer in rum. On a cursory view it might be taken for an extraordinary kind of sea-serpent, and this idea would be even supported by a closer examination of parts of its structure.

It offers points of discrepancy, however, from the several genera of animals nearer allied to it, so important as to entitle it to a distinct place in classification, especially as regards the jaws, which, with the exception of the apparent want of interarticular bones, are truly serpentiform, and from the possession of an enormous elastic sac, which is seemingly a receptacle for air only. The first of these characters seeming to the author least liable to vary, he suggests the term Ophiognathus as applicable to the genus.

He then proceeds to give a technical zoological description of the genus, and to state the points in which it essentially differs from the genera nearer allied to it, after which he gives a more special description.

Its body is of a uniform purplish black, except the filamentous extremity of the tail, which is much lighter. Its total length 4 feet

inches. Its enlarged and extremely elastic pharynx commences with the enormous sac, or air-vessel, which extends about 20 inches from the snout. It is so delicate that it could not be fully inflated, but when partially so, measured about 9 inches in circumference, below its union with the tail, and its greater diameter, including the slender body of the animal, 4 inches.

At about an inch from its termination it is perforated by the rectum, and its tenuity rendered that and the other intestines easily traced. They appear to be sustained beneath the very imperfect ribs by a membranous expansion, not being affected in position by inflation of the sac.

The skin all over the body is particularly soft and shining. The spiracula are large, irregularly oval, and unprovided with external rays, and their edges partially conceal on each side three tufted branchiæ, at about 5½ inches from the snout. The fins are all very small. In the pectoral ones is a peculiarity not found in other apodal genera, being chiefly composed of an adipose disc, which is terminated and nearly surrounded by a narrow radiated membrane, instead of the latter originating from the body immediately. The tail, after the termination of the dorsal fin, becomes a slender tape-like filament, nearly 20 inches longer; and above this point some minute filaments take their growth from it. The anal fin commences where the sac terminates, and ends 14 inches from the end of the tail.

The most curious parts of the animal are, however, the head and jaws. It has no tongue; the teeth are disposed in a single row above and below. The ossa palati are destitute of teeth, and the jaw-bones are so long, and their articulation so loose, as to be susceptible of distention beyond any other animal, not excepting the rattle-snake; and when fully distended they describe a large circle, and appear but the opening of an ample sac.

The author illustrates his description by three figures, and concludes by observing that the Ophiognathus is one of the most voracious of the inhabitants of the ocean. Its entire form would indicate great swiftness of motion, performed doubtless by the same sinuous inflections in water, which excite our admiration in serpents on land. In what manner its pouch is employed in its economy he does not attempt to determine, nor to decide whether it be capable of secreting the contained air, or whether water be allowed to enter it.

An Examination into the Structure of the Cells of the Human Lungs; with a view to ascertain the Office they perform in Respiration. By Sir Everard Home, *Bart. V.P.R.S. Illustrated by Microscopical Drawings from the Pencil of* F. Bauer, *Esq. F.R.S.* Read February 8, 1827. [*Phil. Trans.* 1827, *p.* 58.]

The author commences this paper by remarking that the subject of respiration has been hitherto regarded as belonging rather to chemistry than to anatomy; but that he finds reason to believe that process to be more simple than is imagined, and more within the

reach of anatomical than chemical investigation. The present theory among chemists, he states, is, that respiration decarbonizes the blood, a volume of oxygen and nitrogen being at each inspiration received into the lungs, and returned, measure for measure, the oxygen only being partly converted into carbonic acid; thus proving, as they suppose, that no part of the atmospheric air breathed is retained.

This theory he considers as satisfactory, supposing it supported by the structure of the lungs themselves, and taking it for granted that the blood requires no other change for its purification. But when it was devised, no accurate examination of the cellular structure of the lungs had been set on foot; and it is the object of this paper to explain their mechanism, and see how far it is fitted for the processes this theory requires.

The author began by inquiring into the circulation of blood through the lungs; and by the aid of Mr. Russel procured injections of their veins and arteries, capable of being rendered objects of microscopic investigation by Mr. Bauer.

The first fact discovered was, that though an injection introduced by the pulmonary artery was found to return by the trunks of the pulmonary veins, yet, when thrown in by the veins, it does not return by the arteries.

The next fact ascertained was, that the distention of the air-cells produces an interruption between the arterial and venal circulation, the blood being carried no further than the small arterial branches surrounding the air-cells.

The author then proceeds to describe the air-cells and parts surrounding them, from drawings made by Mr. Bauer. The branches of the pulmonary artery, accompanied by larger and more numerous branches of the pulmonary vein, are seen ramifying behind the internal membrane of each cell. The latter have valves at regular intervals, and there are also numerous absorbents supplied also with valves. The injection was found to have stopped short of the termination of the artery, and the space beyond to be filled with gas.

After this description, and after expressing his obligations to Mr. Dollond for his improvement in the microscopes used by Mr. Bauer, the author proceeds to deduce his conclusions, which are unfavourable to the received doctrine of simple decarbonization, conceiving the structure thus developed as better adapted to receive supplies from the atmosphere, and transmit them to the heart. He considers that the carbonic acid, detected by Professor Brande, in urine and perspirable matter, must have been formed in the blood circulating through the arteries, and have derived its oxygen from the lungs. He considers, further, that the carbonic acid carried off in respiration, is furnished from such venous blood as has acquired it during the process of digestion; having known on a former occasion that soon after that process has been begun, the oxygen employed in it unites with carbon.

He finally remarks that, for carrying on the functions of life, and for supplying oxygen and removing carbon, respiration is necessary,

but is not required for the simple continuance of life when no action is going on; and illustrates this opinion by the instance of the common garden snail.

Remarks on a Correction of the Solar Tables required by Mr. South's *Observations. By* G. B. Airy, *Esq. M.A. Fellow of Trinity College, Cambridge, and Lucasian Professor of Mathematics in the University of Cambridge. Communicated by Dr.* Young, *F.R.S., &c.* Read February 15, 1827. [*Phil. Trans.* 1827, *p.* 65.]

The discordancies observed by Mr. South between the sun's right ascension, as deduced from observation, and those given in the Nautical Almanac, follow a law so simple as not to allow of their being regarded as errors of observation, or arising from any casual cause, but justify us in attributing them to imperfections in the solar tables, with the exception of three days, in which there seems to be some ground to suspect error of computation.

A single inspection of these discrepancies, Mr. Airy observes, suffices to show that they arise almost entirely from an error in the epoch, and an error in the place of the perigee. From the peculiar form of the tables in Vince's Astronomy, which give great facility to the introduction of an error in the excentricity, he was induced at first to suspect that one might exist; but on calculation found the error in the equation of the centre so small as to be entirely insensible. He then proceeds to detail the process by which, from Mr. South's observations, he has deduced the amount of the several errors, which consist in regarding the epoch, the mean anomaly, and the equation of the centre, as erroneous by three very small unknown quantities, and forming as many equations of condition for determining them as there are observations. These combined and resolved, so as to give the most probable result, lead to the conclusions, first, that the correction of the equation of the centre is evanescent; secondly, that the epochs of the sun must all be increased by 9″, and the epochs of the perigee each by 1′ 48″.

On the mutual Action of the Particles of Magnetic Bodies, and on the Law of Variation of the Magnetic Forces generated at different Distances during Rotation. By S. H. Christie, *Esq. M.A. F.R.S.* Read February 15 and 22, 1826. [*Phil. Trans.* 1827, *p.* 71.]

The results obtained by the author, described in a former communication, when a copper disc was made to revolve under a magnetized needle, appearing to him not likely to lead to an accurate knowledge of the law of magnetic attraction, developed during rotation, from the effect of lateral attraction; he was induced to resume the inquiry, substituting a ring for a disc, expecting that, as no lateral forces would here be called into action, the results would be more uniform, and in this expectation he was not disappointed. One of the first phenomena encountered by him in this research, was a very

great diminution of magnetic force, when a ring of the same weight was substituted for a disc; and pursuing this point of inquiry, he found that in all cases of solution of continuity, not only by cuts in the direction of radii from the centre, but in concentric annuli or otherwise, there is always a great loss of force, the magnetism of the whole being always much greater than the sum of that of the parts. He describes in detail these experiments, and the apparatus used for them. His method of estimating the intensity of the force developed, was by suspending the body, set in rotation by a revolving magnet, by a wire, and preserving a constant velocity of rotation in the magnet, to note the time when the velocity acquired by the disc, was just destroyed by the torsion of the wire, and the disc just began to revolve in a contrary direction. Applying analysis to the dynamical problem arising, he thence deduces the intensity of the force urging the suspended body. Applying the resulting formula to the experiments, he finds, in almost all cases, a small diminution in the intensity of the force as the arc of rotation increases, which he attributes to a very minute degree of magnetism accumulated in the disc, and retained by it till the revolving magnet comes round again.

In reasoning on the experiments detailed, Mr. Christie concludes that the greatest developement of magnetism in a disc, subjected to the action of revolving magnets, takes place when the axes of the magnets are vertically under points bisecting the radii, and that the magnetism decreases very rapidly as they approach the edge; thus indicating that for a full developement of magnetism, a continuity of substance, in all directions from the point acted on, is principally requisite. This result is corroborated in a striking manner by the effect produced by concentric circular cuts in the disc, leaving the interior attached to the exterior in several places. On successively destroying these points of connexion, a very great diminution of force is perceived.

The effect of removing, by a circular concentric cut, the interior of a disc, appears to be, to destroy or prevent the developement of a quantity of force directly proportional to the mass removed, the magnets acting at a constant difference from the centre; and reasoning from this and other phenomena, Mr. Christie concludes that the reduction of the disc, by concentric and radiating cuts, into very small portions, though not actually to powder, would render its magnetism quite insensible.

The author next proceeds to investigate, by experiments of the same kind, the law of variation of the magnetic force regarded as depending on the distance of the revolving magnets from the suspended body, which in this case was a cylindrical annulus of copper, about 1 inch in breadth, 10 inches in internal diameter, and $\frac{1}{4}$ inch thick, weighing 32·375 ounces troy; and the axes of the magnets were made to revolve during the whole series of experiments, with an uniform velocity of five turns per second, exactly under the middle of the breadth of the annulus, being fixed vertically with their south poles upwards. He assumes, first, that the action of each magnet may be referred to a single point near its extremity; secondly, that the action may

also be referred to a single point or pole in the copper ring, somewhat in arrear of the point vertically over the magnet, as a consequence of the principles proposed by other writers on the same subject; and, lastly, that the mutual action of these poles on each other is inversely as the 4th power of their distance. Assuming then a formula with indeterminate co-efficients expressive of these conditions, and determining them by comparison of the assumed with the observed forces, he finds that the effect of a variation of distance from $\frac{1}{2}$ an inch to $2\frac{1}{2}$ (which causes a diminution of force from 1982·5 to 11·375, the ratio of nearly 200 to 1,) can be represented within a 40th of the whole force, in the extreme case where the observations are liable to the greatest errors, and in all other cases to the 100th part of the force; so that he considers this law as established by experiment. The same operations give the values of the assumed constants; and it may be remarked that these agree very nearly as deduced from different experiments, and that as a mean result, we may state the distances of its pole from the extremity of each magnet, at 105 thousandths of an inch; and the distances by which the pole of the disc is in arrear of that of the magnet, with the velocity employed, at about four tenths of an inch; and the introduction of this element gives a considerable increase of coincidence between observed and computed results.

The author next proceeds to apply similar processes of assumption and calculation to the case where the magnets were made to revolve horizontally under the ring, with their poles of the same name adjacent. In this case his observations also lead to the conclusion of a law of force, varying inversely as the 4th power, instead of the inverse square of the distance, between the poles of the magnets and the corresponding poles in the ring.

He now reversed the experiments, suspending the magnets over the ring in a vertical position, and making the ring revolve below them till the magnets had attained a state of equilibrium, between the force of the disc in one direction, and that of torsion and their own directive force in the opposite. The same result is still obtained from this experiment, viz. a variation of the force as the inverse 4th power of the distance.

Mr. Christie next enters into an analytical investigation, having for its object to ascertain how far the principle of time being required for the developement of magnetism will account for the phenomena; and the conclusion to which he arrives is, that it will do so satisfactorily. In the course of these investigations, he is led to conclude that, in certain cases, a retrograde rotation in the suspended disc might take place; and suggests the great confirmation such a fact, if observed, would afford to this theory.

Corrections to the Reductions of Lieutenant Foster's *Observations on Atmospherical Refractions at Port Bowen; with Addenda to the Table of Magnetic Intensities at the same Place. By Lieutenant* Henry Foster, *R.N. F.R.S.* Read March 22, 1827. [*Phil. Trans.* 1827, *p.* 122.]

The error of which this paper contains a correction, consists in the adoption of one exact second in arc, for the value of a division of the long level of the repeating circle with which the observations were made by the author for determining the amount of atmospheric refraction at Port Bowen, an account of which is published in the Transactions of the Royal Society for 1826, Part IV.; whereas the true value is 10″·9 in arc. The error was discovered by Captain Kater, who had occasion to use the instrument, and by him communicated to the author, who immediately took measures to ascertain the fact; and having, in Captain Kater's presence, convinced himself of its reality, hastened to make the necessary recomputations of the Tables affected by it, and to submit them, so corrected, to the Society; explaining at the same time the error to have arisen from erroneous information, given him at the time of receiving the instrument, which being the same with that used by Captain Sabine in his pendulum experiments, he took no steps to examine the point; as it never once occurred to him that there could be a doubt on the subject. The Tables affected by this error are the 7th, 8th, 9th, 10th, 11th, and 12th; and the corrections, which fortunately in the three former Tables do not exceed 6″, and in the three latter do not amount to 2″, are stated.

The author observes, in a postscript, that the observations given by Lieutenant Ross, at the end of the paper alluded to, must be now wholly rejected, in consequence of the original observations involving the erroneous datum dependent on the level being left on board the Fury at the time of her loss.

He concludes by requesting the Society's acceptance of the thermometer employed by Captain Parry and himself in the observations in question, which was prepared with every possible care by Mr. Daniell for the purpose.

Corrections of an Error in a Paper published in the Philosophical Transactions, entitled, "On the Parallax of the Fixed Stars." By J. F. W. Herschel, *Esq. F.R.S.* Read February 22, 1827. [*Phil Trans.* 1827, *p.* 126.]

This notice is destined to rectify the consequences of an error in a formula deduced in the paper alluded to, which was pointed out in the errata to the Fourth Part of the Philosophical Transactions for 1826, by the re-computation of the table accompanying it, which table so re-computed is annexed.

On a new Form of the Differential Thermometer, with some of its Applications. *By* William Ritchie, *A.M. Rector of Tain Academy. Communicated by* J. F. W. Herschel, *Esq. Sec. R.S.* Read December 21, 1826. [*Phil. Trans.* 1827, *p.* 129.]

The instrument described by Mr. Ritchie in this paper, consists of two hollow cylinders of tin-plate, of large diameters in proportion to their height, placed with their bases parallel to each other, and at a moderate distance, and connected by a glass thermometer-tube, containing a coloured liquid, and in the form of an inverted syphon, after the manner of the photometer described by the same author in the Philosophical Transactions for last year. This instrument being placed between two sources of radiant heat, at such distances that the coloured liquid shall remain stationary in the tube, fixes the distances of equal radiation, from whence the radiations at equal distances may be calculated, supposing the law of the decrease of heat by radiation known, and *vice versâ*, supposing the ratio of the heats radiated at equal distances known, the law of radiation may be experimentally investigated.

As instances of the application of this instrument to experimental purposes, the author relates several experiments on iron balls, equally heated, and exposed at such distances as to subtend equal apparent diameters at the faces of the cylinders. From these he concludes that the law of the decrease of heat, as the inverse squares of the distances, is founded in fact. When, however, the heated bodies exposed had flat surfaces corresponding to those of the surfaces of the cylinders, he found a less rapid law of decrease to hold good; from which he concludes, that a constant portion of heat is radiated directly out from the surface without divergence, because an equal quantity, added to both terms of a ratio of greater inequality, as is well known, diminishes the ratio.

On the Structure and Use of the Submaxillary Odoriferous Gland in the Genus Crocodilus. *By* Thomas Bell, *Esq. F.L.S. G.S. Communicated by Sir* Everard Home, *Bart. V.P.R.S.* Read March 1, 1827. [*Phil. Trans.* 1827, *p.* 132.]

The author begins by remarking on the general inattention prevailing among anatomists, to such glands as produce anomalous secretions, required only by the peculiar and exclusive habits of the animals possessing them. A gland of this kind subsists beneath the lower jaw of the alligator and crocodile. It is situated on each side, and secretes an unctuous substance of a strong musky odour. Neither its structure nor its probable object have yet been considered with any care, till the author, about two years ago, discovered in it a structure which he thinks is without a parallel in the glandular system of other animals. His observations were made on the common alligator of America. In this animal the external orifice of the gland is situated

about two thirds of the length of the lower jaw backwards from the symphysis, being a longitudinal slit a little within the lower edge of the basis of the jaw, and through it exudes an unctuous substance of the consistence of suet and the smell of musk. During warm weather, when the animal feeds freely, the secretion is copious; but in winter is much diminished in quantity, and less powerful in scent.

The gland, on removing the integuments, is seen lying between the skin and under-surface of the tongue. It consists of a simple follicle or sac, of a blueish colour, and an elongated and pyriform shape. In an alligator four feet in length, it is about half an inch long and one sixth in diameter. It is lined with a soft secreting membrane.

The gland is enveloped by extremely fine, delicate, muscular fibres, disposed obliquely, and consisting of two fasciculi, passing respectively over and under the gland, and uniting at its base into a long slender round muscle, which, after making a slight curve forwards, proceeds directly back to the corner of the os hyoides, to which it is closely united; and following the course of another muscle apparently identical with the mylo-hyoideus in mammifera. The use of the muscle seems to be to bring the gland into a proper position for discharging its contents, and to operate such discharge by its pressure.

The author, taking into consideration the situation of the gland near the mouth of the alligator, its predatory habits and voracity of fish, and the well-known partiality of fish for odoriferous oils and extracts, conceives that the use of this secretion is to act as a bait, and attract the fish to such a position that he can easily seize on them, in his usual way of seizing his prey, by snapping sideways at them.

On the Permeability of Transparent Screens of extreme Tenuity by radiant Heat. By William Ritchie, *A.M. Rector of Tain Academy. Communicated by* J. F. W. Herschel, *Esq. Sec. R.S.* Read March 8, 1827. [*Phil. Trans.* 1827, *p.* 139.]

The author states in this paper, that invisible radiant heat, from sources at elevated temperatures, freely permeates thin transparent screens in the same manner as light; but that as this doctine, established by Profesor Prévost and M. de la Roche has been controverted, he thinks it necessary to demonstrate it by fresh experiments: to this end he covered a small aperture with a film of glass almost iridescent, and keeping it constantly cold, by blowing on it, below the temperature of ambient air, he found that an air-thermometer on one side of it was not affected by a heated iron ball on the other, if the temperature of the ball was low; but that as this temperature was raised, though not to the point of visible ignition, the effect on the thermometer became sensible and even considerable.

In another experiment, two air-thermometers, having their bulbs transparent, and as thin as possible, were placed equidistant from a heated ball just ceasing to be visible in the dark. The one was clear,

the other *coated inside* with a thin film of pounded charcoal. The latter was most affected.

In a third experiment, a frame of glass threads, or fine wire, placed vertically, was coated with a film of diluted white of egg, applied with a broad hair brush, and kept constantly at the same temperature by applying it fresh and fresh. Then, exposing a heated ball on one side, and an air-thermometer on the other, no effect was sensible on the latter, when the temperature of the ball was low, but when just invisible in the dark the effect was very sensible.

The author also finds that this effect is greater than in the case of glass, and that liquid screens are more permeable to heat than solid ones. He also found that little difference of effect is observed whether the screens be near to or far from the heated ball, *cæteris paribus*; and this he considers as demonstrating that the effect was not due to secondary radiation from the screen.

On the Derangements of certain Transit Instruments by the effects of Temperature. By Robert Woodhouse, *A.M. F.R.S. &c.* Read April 26, 1827. [*Phil. Trans.* 1827, *p.* 144.]

In the Philosophical Transactions for 1825, the author alluded to the derangement of the Cambridge transit instrument, arising from unequal expansion of its braces, establishing, as he conceived, the fact and cause of such derangement; and in a subsequent paper instanced its effect in one case as altering, by no less than 20″, the time of the passage of the pole star over the wires. In consequence the removal of the braces was resolved on, but from one cause or other delayed; but the author considers good to have arisen from this procrastination, as enabling him to make further experiments, which he was led to do in consequence of Mr. South's observations, which lead to conclusions opposite to those deduced by himself. To satisfy his own mind, therefore, he instituted the series of experiments described in this paper.

His first care was to determine precisely, by a series of transits, the polar intervals between the wires of his eye-piece. He then observed the pole star at its lower culmination, and after its passage over the middle wire, applied a warm blanket to the upper eastern and lower western brace, and found that a deviation of the telescope to the west had taken place, such as to alter the passage over the remaining wires nearly 19 seconds, and in the direction corresponding to the expansion of the braces. Another observation, under more favourable circumstances, gave a similar result; viz. 18 seconds of retardation.

In another experiment the warm blankets were applied to the upper western and lower eastern braces, when deviations appeared to have taken place to the extent of 36, 29, and 27 seconds in the respective passages over the 5th, 6th, and 7th wires.

In another trial the passages over the three first wires were ob-

served, and warm blankets were then applied to both the western braces, and a deviation to the west, though not above one third of that arising from their application to the alternate braces was the consequence, indicating the difference, not the sum, of the actions in the two cases; and in this experiment it is very unlikely that the two braces should have been equally heated.

The next experiment was varied by observing the passage over the 1st and 2nd wires, then warming the upper western brace, and observing it on the 3rd, 4th, and 5th wires, allowing the instrument to cool, then again warming the *lower* western brace to a yet greater degree, and observing on the 6th and 7th wires. The result was a deviation to the east, caused by the first warming, a return to its mean state, and then a deviation to the west from the second warming.

The experiments were varied by holding the braces some time in the hand, and with similar results.

From these details, the author concludes that the partial heating of the diagonal braces, or of any one of them, deranges the Cambridge transit instrument, according to the reasoning in his former paper; and that this cause may, in certain instruments, and under certain circumstances of temperature, produce balancing effects,—thus giving an appearance of inflexibility which, under other circumstances, would not subsist.

He then enters into a consideration of the circumstances of Mr. South's experiments, in which, as the braces on the same side were equally heated, the difference of temperature in the upper or lower parts of the tube alone could have operated, and might produce an insensible effect. He combats the idea that the apparent rigidity of Mr. South's instrument arose from its peculiar construction; as the Greenwich transit, which is similar, and by the same artists, has been found by the assistants, on holding the alternate braces in their hands, to undergo a considerable deviation by the test of the meridian mark. But to be more sure of the nature of the result produced, he requested the Astronomer Royal to try with the Greenwich transit the first experiment mentioned in this paper. He did so, and the results were found to be in accordance with the views entertained by Mr. Woodhouse, and are here stated. He concludes this subject by inviting Mr. South to an experiment, decisive, as he conceives, of the question as far as concerns his instrument, viz. to observe the passage of Polaris in October with one brace exposed to the sun.

On the subject of the sun's transits, he declares himself unwilling and unprepared to enter, and states himself to have found a difference between the clock's errors, as determined by the sun and the stars, similar to that concluded by Mr. South, and nearly the same in quantity. This, he says, may be partly explained by the increase of the right ascensions of the stars by three tenths of a second, in late catalogues by Mr. Pond, while no corresponding change was made in the catalogue from which the solar tables were computed.

He considers, on the whole, that the differences in question arise

partly from errors in the solar tables, and partly from instrumental derangement.

On some of the Compounds of Chromium. *By* Thomas Thomson, *M.D. F.R.S. L. and E. Professor of Chemistry, Glasgow.* Read March 29, 1827. [*Phil. Trans.* 1827, *p.* 159.]

The principal object of this paper is to give an account of a singular compound of chromic acid and chlorine, discovered some years ago by the author; but in the investigations to which it gave rise, the author was led to a more careful examination of the oxides of chromium than they had before undergone, and to a knowledge of their composition. An account of these researches he therefore proposes to give in this communication. He begins by describing metallic chromium. That used by him was reduced by Mr. Cooper: it was white, with a shade of yellow, very brittle, not sensibly attracted by the magnet even in fine powder. Its specific gravity was 5·093. Nitric acid boiled on it has no effect, and aqua regia scarcely any, unless the action be very long continued. When heated, however, with a mixture of potash and nitre, it is converted into chromic acid; 3·14 grains of the metal, thus treated, yielded by solution and precipitation 16·23 of chromate of lead, giving for the weight of an atom of chromium 3·966, or in round numbers 4·000. The author's stock of metallic chromium was so small as to prevent the repetition of the experiment.

The author next describes the green oxide of chromium. This is easily produced by the action of de-oxygenizing agents, such as alcohol, sulphurous acid, or sulphuretted hydrogen, on chromate of potash. When thus obtained it is in the state of a hydrate, containing [illegible]ths of its weight of water, and easily soluble in acids. A moderate heat, however, expels the water, and leaves the oxide insoluble in any acid. When further heated nearly to redness, it glows, or becomes of itself suddenly intensely red hot. Its atomic weight cannot be determined from its salts, as it forms none,—at least crystallizable and definite enough for the purpose; but as we know that of chromic acid to be 6·5, if we can determine the number of atoms of oxygen to be abstracted to convert it into green oxide, that of the latter will be known. To this end the author deoxidized the chromate of potash by sulphuretted hydrogen. Hydrosulphuret of chromium, composed of its ingredients, atom to atom, fell in the state of a green powder soluble in acids. The liquor, after driving off the redundant gas by heat, was found to be a solution of hyposulphite of potash. To avail himself of this fact, however, it became necessary to investigate the composition of the hyposulphurous acid. This he effected as follows:—he first analysed a crystallized hydrosulphuret of soda, formed on a large scale in certain soda-leys, which he found to consist of 1 atom bisulphuretted hydrogen, + 1 atom soda, + 6 atoms water. Through a solution of this salt he passed sulphurous acid, which converted it into hyposulphite, and threw down just half

the sulphur contained in the salt. The hyposulphite of soda thus obtained gave by analysis a per-centage of hyposulphurous acid, agreeing with 5 as its atomic number, on which supposition it must be regarded as consisting of 2 atoms sulphur, + 1 oxygen; and this composition he states himself to have verified by direct analysis of several hyposulphites. From this it is easy to derive the composition of the green oxide of chromium, the weight of which thus comes out equal to 5.

After the description of two compounds, the one of chromic acid and oxide of chromium, and the other of sulphur and the same oxide, obtained by a variation of the circumstances under which sulphuretted hydrogen is made to act on the chromic salts, the author proceeds to relate experiments in which protosulphate of iron was used as the deoxidizing matter. When a solution of this salt is mixed with one of chromate of potash, a precipitate falls, consisting of green oxide of chromium and peroxide of iron, which, from considerations and experiments detailed at large in the paper, appeared to consist of four atoms peroxide of iron, and one of green oxide of chromium.

Phosphuret of chromium was formed by subliming phosphorus through red-hot green oxide. It is stated by Dr. Thomson to consist of 1½ atom of phosphorus, and 1 atom of the metal. This phosphuret, by digestion in nitric acid, was converted into phosphate, consisting, according to the author, of 1 atom protoxide, and 1¼ atom phosphoric acid. No sulphuret could be formed by a similar process. The author devotes the next section of his paper to a detail of several unsatisfactory experiments for determining the atomic weights of chromic acid and the protoxide, by various processes, in which only partial decompositions were effected.

He next treats of the brown oxide. It is prepared by passing sulphurous acid through solution of chromate or bi-chromate of potash. It does not combine with acids; and when treated with acids, alkalies, or even water, resolves itself into chromic acid and protoxide, of which it appears to be either a mixture or a compound, far from intimate, in the proportion of 1 atom acid to 6 atoms oxide.

The next section of this paper is devoted to an account of the chloro-chromic acid, a remarkable compound produced by making sulphuric acid act on a mixture of 190 parts of bichromate of potash, and 225 parts of common salt. From this mixture, on applying heat, it separates in red fumes, and distils over in a liquid of a rich deep crimson colour, of a sweet astringent acid taste, and strong smell of chlorine. Specific gravity nearly twice that of water, with which it does not mix, but which decomposes it, evolving chlorine and producing heat. This liquid dropped into oil of turpentine or alcohol, or when poured on sulphur, sets them on fire; but (what is remarkable) it not only does *not* fire phosphorus, but even extinguishes it when already inflamed. On other combustibles and metallic bodies it acts with great energy, but without producing ignition.

In ammoniacal gas, however, it burns vividly. When heated *per se*, the chlorine escapes, and a substance resembling green oxide re-

mains. Dr. Thomson analysed it by solution in water, saturation with carbonate of soda, and precipitation by solutions of baryta and silver; and states its composition from such analysis, to be chromic acid and chlorine, atom to atom.

The fifth section of this paper is devoted to an account of the salts of chromium. They are formed by the union of the green oxide with acids, and are all uncrystallizable, and of very intense colours. They are not precipitated by sulphuretted hydrogen. Gallic acid precipitates them green. Prussiate of potash only changes their colour to brown, and throws down no precipitate. Ammonia and potash throw down green oxide, which re-dissolves in excess of the latter.

The muriate of chromium always contains an excess of acid, and is deliquescent. When this is driven off it becomes a chloride, and is insoluble in water and in acids.

The nitrate also contains an excess of acid, which cannot be neutralized by adding more oxide. Dried and slowly heated, it is chiefly converted into chromic acid contaminated with a little green oxide.

The sulphate also reddens vegetable blues. It consists of the acid and oxide, atom to atom, and 3 atoms of water.

The precipitate from muriate of chromium by carbonate of soda, is a di-carbonate with 4 atoms of water. When bi-carbonate of potash was the precipitant, a penta-carbonate was obtained, which the author regards as a fact not easily explained.

The bi-phosphate precipitates from the muriate on adding phosphate of soda: it is a lively deep green powder; when dried in a sand heat it contains 5 atoms of water. Neutral phosphate could not be formed. The arseniate is very nearly neutral, but a bin-arseniate also exists.

Chromic acid dissolves protoxide, and forms a chromate. This is precipitated when muriate of chromium and chromate of potash are mixed, and is of a brown colour, and soluble in water.

After describing other salts, as the oxalate, tartrate, and potash-tartrate, Dr. Thomson proceeds to give an account of certain chromates not before described. These are the per-chromate of iron, the di-chromates of lead and silver, and the double chromates of potash and soda and of potash and magnesia.

The author concludes this paper with an account of his analyses of the mineral compound known in cabinets as chromate of iron, which, when examined in a state of purity, he found to consist of 2 atoms of green oxide of chrome, 1 of peroxide of iron, and 1 of alumina, together with a minute admixture of a white matter, apparently a metallic salt, of unknown acid and base, and which, though accompanying specimens from several localities, yet in all was too small in quantity for thorough examination.

Rules and Principles for determining the dispersive Ratio of Glass; and for computing the Radii of Curvature for achromatic Object-Glasses, submitted to the Test of Experiment. By Peter Barlow, *Esq. F.R.S. Mem. Imp. Ac. Petrop. &c.* Read May 3, 1827. [*Phil. Trans.* 1827, *p.* 231.]

The author begins this paper by an enumeration of the various works on the subject extant in our language, and a general mention of the writings of foreign mathematicians, which he considers as leaving room for further inquiry and simplification. He then states the method employed in his experiments for determining the refractive and relative dispersive powers of his glasses, the former of which is that generally known and practised;—of measuring the radii and focal length of a lens, and thence deriving the refractive index; with some refinements in its practical application, consisting chiefly in using the lens as the object-glass of a telescope, and adapting to it a positive eye-piece and cross-wires, which are brought precisely to the true focus by the criterion of the evanescence of parallax arising from a motion of the eye, as is practised in adjusting the stops of astronomical instruments. The only source of error it involves is in the measurement of radii of the tools which it was found could always be performed within $\frac{1}{500}$th of their whole values. The dispersive ratio of two glasses was determined by over-correcting the dispersion of a convex lens of the less dispersive glass by a concave of the greater, and then withdrawing the latter from the former till the achromaticity is perfect, or as nearly so as the materials will admit, and measuring the interval between the lenses and their foci, from which data the ratio of their dispersive powers is easily obtained.

The refractive indices and dispersive ratio thus determined, the next step is to find the radii of curvature so as to destroy spherical aberration. In this investigation, the author does not consider it as necessary to limit the indeterminate problem by any further condition, as others before him have done, but regarding it as a matter of great convenience to avoid contact of the interior surfaces in the centre of the glasses, leaves it open to the optician to make a choice within certain limits, thus avoiding what he considers as an intricate equation arising out of the fourth condition. He proceeds, therefore, to express analytically the aberrations of the glasses, and to deduce the equation expressive of its destruction, which of course involves one indeterminate quantity; this may be either of the radii, or any combination of them. The author chooses the ratio of the radii of the interior and exterior surfaces of his flint lens for this indeterminate, which he assumes, as well as may be, to satisfy the condition of the absence of contact and near equi-curvature of the adjacent surfaces; thence deduces, first, the radii of both of the surfaces of the flint lens; next, its aberration to be corrected; and thence, by the solution of a quadratic, or by the use of a table containing its solutions registered in various states of the data, the ratio

of the radii of the convex, whence the radii themselves are easily deduced.

Mr. Barlow next inquires into the validity of the empirical rule employed by Mr. Tulley, as stated in Rees's Cyclopædia, which in many usual cases he finds to give results nearly agreeing with his own computations; in others, however, it differs too widely to be depended on.

The author next enters on an experimental inquiry of the limits within which an error in spherical aberration or dispersion may have taken place without producing a sensible defect in the object-glass, by procuring, with the assistance of Mr. Gilbert, glasses to be ground to radii nearly, but not quite agreeing with the results of computation. It results from them, that in some states of the data and assumed radii, much greater deviations may be borne than in others, and the author considers that such combinations should be preferred as admit the greatest latitude in this respect.

The author concludes this paper by a synoptic statement of a mode of approximate solution applicable (in consequence of the peculiarity of the formula for the destruction of the spherical aberration) to all ordinary states of the data, and comprised in a very short and easily calculable form; and by a method of practically determining the curvatures and indices of refraction of any given convex or concave lens.

On the Change in the Plumage of some Hen-Pheasants. By William Yarrell, *Esq. F.L.S. Communicated by* William Morgan, *Esq. F.R.S., March* 19, 1827. Read May 10, 1827. [*Phil. Trans.* 1827, *p.* 268.]

The last shooting season having been unusually productive of hen-pheasants, which have assumed more or less the plumage and appearance of the male, much discussion in consequence has arisen on the cause of this change; and the author having had many opportunities of examining the facts, both as respecting the pheasant and domestic fowl, was induced to notice the internal peculiarities which invariably accompany this transformation. According to an opinion of Mr. J. Hunter and of Mr. Butter, this change only takes place at an advanced age; but the author considers the facts in his possession as at variance with this idea, and that the appearances in question may occur at any period of life, and may even be produced artificially.

In all the instances examined by him, the sexual organs were found diseased, and to a greater or less extent in proportion to the change of plumage. The ovarium was shrunk, purple, and hard; the oviduct diseased, and the canal obliterated at the upper part, immediately preceding its funnel-shaped enlargement at the bottom of the ovarium. Having opened a hen-pheasant in its natural plumage, for the sake of comparison, he found a similar diseased state of the organs to exist; thus proving that the disease must exist some time before the corresponding change of feathers takes place.

He then observes, that it is no uncommon thing to find, among numerous broods of pheasants reared by hand, some females, which, at the age of only four months, produce the brightest plumage of the male; and in two instances of birds shot in a wild state, the nest feathers had not been shed, proving them to have been birds of the year.

A partridge, having a white bar across the breast, and the first three primary feathers in each wing white, being opened, exhibited the same sort of organic disease; and from circumstances adduced, it appears that this was also a bird of the year.

All variations in plumage, however, are not traceable to this cause. In most of the excepted instances, however, the individuals are dwarf birds, and the author attributes their variety of plumage to defective secretion,—the effect of weakness.

When the sexual organs are artificially obliterated in the common fowl:—in the male bird, so soon as this operation is performed, he ceases to crow; the comb and gills do not attain their full size; the spurs remain short and blunt; and the feathers of the neck assume an appearance intermediate between the hackled appearance of the cock, and the ordinary web of the hen. The operation on the female being performed (by obliterating the oviduct), the ova cease to enlarge; she makes an imperfect attempt to crow; the comb increases in size; and short and blunt spurs make their appearance. The plumage also alters both in colour and form, and approaches that of the cock; and the bones of the lower part of the back never acquire that enlargement requisite for giving a proper breadth to the pelvis. In short, the two sexes by this process approximate so nearly in character, that it is frequently difficult to determine the sex.

In the case of hen-pheasants, they assume the plumage of the male at best but imperfectly, and it is probable that they do not live many years after the change.

The author concludes by regarding it as a general law, that where the sexes of animals are indicated by external characters, these undergo a change, and assume a neutral appearance whenever original malformation, subsequent disease, or artificial obliteration, has deprived these organs of their true influence.

On the secondary Deflections produced in a Magnetized Needle by an Iron Shell, in consequence of an unequal Distribution of Magnetism in its two Branches. First noticed by Captain J. P. Wilson, *of the Honourable East India Company's Ship Hythe. By* Peter Barlow, *Esq. F.R.S. Mem. Imp. Sc. Petrop.* Read May 17, 1827. [*Phil. Trans.* 1827, *p.* 276.]

Captain Wilson being engaged in the prosecution of Mr. Barlow's inquiries as to the laws of the deflection of a needle by an iron shell, had remarked, while in China, that when a magnetic needle was placed in the equator of an iron shell, though no deviation arose when the compass was in its natural state, yet when one end of the

needle was deteriorated, by touching it with the pole, of its own name, of a magnet, a deviation then arose; and communicating the facts to Mr. Barlow, he proceeded to investigate the laws and amount of the deviation so arising.

He distinguishes the action into three several cases:—

1st. When the needle is on any part of the magnetic meridian of the ball. In this case there is no deviation caused by the primary of the shell, nor any secondary deflection produced by deteriorating one pole of the needle.

2nd. When the needle is in the magnetic equator of the ball. In this case he observed, that whichever end of the needle is weakened approaches the ball.

3rd. In every other position, one branch of the needle is nearer to the centre of the ball than the other. Here it is observed, that if the near end is deteriorated, the needle approaches its natural magnetic situation; but if the more distant, then the reverse takes place; and this represents the general law of the phenomenon.

From this law, Mr. Barlow is led to the explanation of the facts, on the principle of induced magnetism in the shell; for in the second case above enumerated, the equilibrium of the needle is produced by equal and opposite repulsions on its equal and equally magnetized ends. When, therefore, the repulsion on one end is weakened by deteriorating that end, that on the other obtains the advantage, and the deteriorated end is drawn towards the ball.

In the general case, the reasoning is equally simple; and Mr. Barlow shows that its results are precisely those which observation affords.

These results he considers as decisive in favour of that theory which regards the magnetism of an iron shell as induced in it by the action of the earth.

He concludes this paper with an account of some experiments instituted for the purpose of determining numerically the amount of the secondary deflections, arising from a given extent of deterioration in the needles. This was estimated by the increase of the times of oscillation in the needle, freely suspended, and the deflections estimated by making the deviations produced by the shell in a series of situations all around it, in various planes; the results of which are stated in a table.

On the Difference of Meridians of the Royal Observatories of Greenwich and Paris. By Thomas Henderson, *Esq. Communicated by* J. F. W. Herschel, *Esq. Sec. R.S.* Read May 17, 1827. [*Phil. Trans.* 1827, *p.* 286.]

Mr. Henderson, in going over the calculations of the observations made by the Commissioners on the part of the British Board of Longitude and the French Ministry of War, for determining this element, in July 1825, detected an error of one second in the reduction of the observations made at the Royal Observatory at Greenwich, from

mean to sidereal time, and which was set down and calculated on by Mr. Herschel in the paper drawn up by him, containing an account of the operation and the results deduced, on the authority of official communication with the Astronomer Royal. This error falls on the reduction of the single Greenwich observation of the 21st of July; and though partly compensated by an opposite error of three tenths of a second committed by Mr. Herschel himself in the reduction of that day's observations, is still sufficient to account for and correct the great and perplexing deviation of that day's results from those of the other three days in which only the signals proved successful.

The effect of Mr. Henderson's correction is, therefore, to redeem the result of the observations of the 21st of July from the suspicion which attached to them; to produce a change of one tenth of a second in the final result of the whole operation, giving $9^{m}\ 21^{s}{\cdot}5$ for the most probable difference of longitude between the two observatories; and, as Mr. Henderson observes, triples the value of the result obtained, by narrowing the extreme range of the experiments from $0^{s}{\cdot}65$ to $0^{s}{\cdot}21$. After a minute re-calculation of the whole work, and the revision especially of the rates of the chronometers, (by which that used at Fairlight appears to have kept a better rate than was at first supposed,) Mr. Henderson concludes his paper with the application of the doctrine of probabilities, to determine the weights of the several observations, and the probable error of the final result, which comes out $0^{s}{\cdot}07$, though the actual uncertainty, he thinks, may amount to $0^{s}{\cdot}2$.

Some Observations on the Effects of dividing the Nerves of the Lungs, and subjecting the latter to the Influence of voltaic Electricity. By A. P. W. Philip, *M.D. F.R.S. L. and E.* Read May 10, 1827. [*Phil. Trans.* 1827, *p.* 297.]

The author, in this paper, first recapitulates the results obtained by him in a paper published in the Philosophical Transactions for 1822; by which it appears that the secreted fluids of animals are so deranged, by dividing the nerves of the secreting organs, as to be incapable of performing their functions; but that they may be restored to their former powers by transmitting voltaic electricity through the secreting organs by the portion of the divided nerves attached to them. In this paper, the functions of the stomach were chiefly considered; in the present, he proposes to consider those of the lungs.

When the nerves of the 8th pair, supplying the lungs, are divided, the animal breathes with difficulty, and speedily dies of suffocation. If the lungs be examined after death, their cells are found so completely filled with a viscid fluid, as to obliterate them entirely, as well as the air tubes. They sink in water; and from a description by Mr. Cutler, which is stated by Dr. Philip at length, it appears that they are rendered impermeable to injections.

The author then states, on his own testimony and that of various other gentlemen who have witnessed the fact, that if the due degree

of voltaic electricity be transmitted through the lungs by those portions of the nerves which remain attached to them, no affection of the breathing supervenes, and the lungs after death are found quite healthy, unless the electricity has been applied of such power, or for so long a time, as to cause inflammation; in which case, the appearances on dissection are those of inflammation, and not those produced by mere division of the nerves.

On the Effects produced upon the Air Cells of the Lungs when the Pulmonary Circulation is too much increased. By Sir Everard Home, *Bart. V.P.R.S.* Read May 31, 1827. [*Phil. Trans.* 1827, *p.* 301.]

In examining the air cells of the lungs of a hare that had been coursed, the author found the superficial large cells filled with colourless coagulable lymph, forming white specks, and the smaller, more interior ones filled with coagula of red blood. No such appearance was seen in the lungs of hares, snared or shot. A run of fifteen minutes with greyhounds so exhausts the hare, that it is frequently known to die from over exertion before the dogs are able to reach it. To examine the state of the lungs, in which the white specks were seen, they were injected with mercury through the bronchiæ, and then immersed in rectified spirits to prevent them from collapsing, and in this state examined microscopically and drawn by Mr. Bauer. The drawings accompany the paper.

The white specks appear to be portions of coagulable lymph, separated from the circulating blood in consequence of its disturbed state, and the author considers them as giving great insight into the nature of that destructive disease called tubercles in the lungs; and in support of this idea quotes Dr. Baillie's description, and refers to his plates of them in his Morbid Anatomy.

Theory of the Diurnal Variation of the Magnetic Needle, illustrated by Experiments. By S. H. Christie, *Esq. M.A. F.R.S.* Read June 14 and June 21, 1827. [*Phil. Trans.* 1827, *p.* 308.]

Mr. Christie having been led to doubt the validity of the explanation of the moving easterly variation adopted by Canton, but at the same time having observed that the changes in deviation and intensity appear always to have reference to the position of the sun with regard to the magnetic meridian, was led to connect these phenomena with Professor Seebeck's discovery of thermo-magnetism, and Professor Cumming's subsequent experiments; and to refer the phenomena of diurnal variation to the effect of partial heating, modified, perhaps, by that of rotation, and by peculiar influence in the sun's rays.

In support of this opinion, he cites passages from papers by Professor Cumming and Dr. Traill, whom a similar idea appears also to have impressed. But in place of looking to the stony strata, of which the earth's surface consists, as the elements of the thermo-magnetic

apparatus which this doctrine requires, the author regards them as rather consisting of the atmosphere and the surfaces of land and water with which it is in contact. Thermo-magnetic phenomena, he remarks, have hitherto only been observed in metallic combinations, but this may be owing merely to the small scale on which our experiments are conducted.

To put to the test of experiment whether thermo-magnetism could be excited when the surfaces of two metals, instead of touching at one point, were in symmetrical contact throughout, the author first employed a compound ring of bismuth and copper, the copper outwards, and he found that to whatever point heat was applied, magnetic powers were developed, a needle being affected differently, according to the different positions in which the ring was placed with regard to it. After a lapse of two years from this first experiment, the author resumed the inquiry with an apparatus consisting of a flat ring of copper, having its inner circumference grooved and united firmly by soldering and fusion to a plate of bismuth cast within it; the whole forming a circular plate 12 inches in diameter, weighing 119 ounces troy, which was made to revolve in its own plane.

Heat was applied by a lamp to a given point at the circumference of this plate, and a delicately suspended needle, partly neutralized, was placed near it, and the deviations observed in all positions of the heated point, which was made to revolve (the lamp being withdrawn). These experiments led him to conclude that the effect of so heating a portion of the circumference, was to create a temporary polarity in the plate, the law of which he explains. He then details a set of experiments, by which he assured himself that a uniformity of action obtained wherever in the circumference the heat was applied. He next instituted a series of observations for determining the laws which govern the magnetic phenomena resulting from the application of heat, as above described, the results of which are stated in the form of tables. Four poles appeared to be produced, two north and two south: both the north lying in one semicircle, and both the south in the other, and not in alternate quadrants, and all the poles lying rather nearer to the centre than line junction of the two metals. The experiments were pursued in a variety of positions of the plate with respect to the meridian and horizon, and with a similar general result.

From these experiments, the author concludes, that uniformity of junction of the two surfaces of a thermo-magnetic combination is no obstacle to the development of transient polarity. Regarding the earth and the atmosphere as such a combination, and limiting our views to the intertropical zone alone, we should have two magnetic poles produced on the northern, and two on the southern side of the equator, similarly pointed, the poles of the opposite names being diametrically opposed to each other.

To apply this to the earth, it is necessary to know the times of greatest heat in the twenty-four hours: this may be assumed at three o'clock in the afternoon. The apparatus used by the author not

affording, when adjusted to the latitude of the place, sufficient magnetic power to render the effects distinct, he substituted for it an artificial imitation, consisting of two magnets six inches long, so placed, with respect to a revolving axis parallel to the axis of the earth, as to imitate the position of the poles produced by thermo-magnetism in his plate; and making the apparatus revolve round this axis, noticed the deviations produced thereby on a compass placed horizontally over it. These deviations he then compares at length with those actually observed, 1stly, by Lieutenant Hood, in 1821, at Fort Enterprise, lat. 64° 28′ N.; 2ndly, by Canton in London, in 1759; 3rdly, by Lieutenant Foster at Port Bowen, in 1825; 4thly, by Colonel Beaufoy at Bushy Heath, in 1820. The results of this comparison are on the whole generally such as to indicate a conformity between the hypothesis and fact, with the exception of some deviations from the exact times of maximum and minimum variation, which could not but be expected. Those observations which afford the least support to the hypothesis are those at Port Bowen; but the case is so extreme, with a dip of 88°, that the author does not regard them as essentially opposed to it, as modifying circumstances must here have an overpowering influence.

The author then considers the manner in which the distribution of land and sea over the globe modifies the point of greatest heat, and in consequence the place of the diurnal poles. He then observes, that at the commencement of his experiments he had no idea of being able to reduce the deviations of the needle to so simple a law as that resulting from a polarity in a particular direction, communicated to the plate, but that he considered it of the greatest consequence to ascertain whether the deviations at the outer edge of his plate had the same general character with those within, at the line of junction of the metals; since these situations of the needle would correspond to great elevations in the atmosphere and points near the earth's surface, respectively as the character of the deviations turns out to be the same in both cases; so that in this respect the hypothesis, so far as is known, agrees with observation.

Mr. Christie proposed prosecuting these experiments with a hollow copper shell filled with bismuth; but from unequal thickness of the copper, and imperfect contact, his experiments proved less uniform and satisfactory in their results. One general effect, however, afforded a striking correspondence with nature. The whole equator being heated, and one part more than the rest, he uniformly found that, the elevated pole being towards the north, the north end of the needle deviated west, when the place of heat was on the meridian above the horizon, and east, when below it; which is precisely the character of the diurnal variation, north latitude.

On the ultimate Composition of simple alimentary Substances; with some preliminary Remarks on the Analysis of organized Bodies in general. By William Prout, *M.D. F.R.S.* Read June 14, 1827. [*Phil. Trans.* 1827, *p.* 355.]

The author commences by observing, that the present is the first of a series of communications, which he hopes to have the honour of laying before the Royal Society, on the same subject; and that the object of the whole series is to determine the exact composition of the three great divisions, viz. the saccharine, the oily, and the albuminous, in which the alimentary matters employed by the more perfect animals may be comprehended; and afterwards to inquire into the changes induced in them by the action of the stomach and other organs, during the subsequent stages of assimilation.

The present paper includes some preliminary observations on the analysis of organized bodies in general, and the composition of the first of the above classes, viz. the saccharine.

After an historical sketch of the principles and progress of the analysis of organized substances, the author makes some remarks on the difficulties attending the use of oxide of copper as now employed. He observes that it is not only hygrometric, but, like many other powders, also condenses air. He likewise found, that when the oxide was removed from the tube in which the combustion had been effected, and retriturated, and reburnt (as in most instances was necessary), it almost invariably gained, instead of losing, weight; a circumstance which he ascribes to the combination of the oxygen of the air contained in the tube, with the partially reduced oxide of copper. These sources of error, from their variable and uncertain character, he found it impossible to ascertain, and was at length obliged to adopt another principle.

When a substance composed of hydrogen, carbon, and oxygen, is burnt in a given quantity of oxygen gas, one of three things must happen; either the volume of the gas will not be changed, in which case the hydrogen and oxygen must exist in the substance, in the proportion in which they form water; or, secondly, the volume may be increased, in which case the oxygen must exceed that proportion; or, lastly, it may be diminished, when the hydrogen must predominate. These well-known facts the author has taken advantage of for determining the composition of vegetable substances, and proceeds to describe the apparatus he employed for that purpose, which consists essentially of accurately graduated syphon gasometers, placed at a convenient distance from one another, so that they may be readily connected by means of an intermediate tube, in which the substance to be analysed is introduced. Heat is applied to this tube by means of a spirit-lamp, and the oxygen gas transferred, by means of mercury, from one gasometer to another, through the ignited tube. After the operation, the difference of volume, as compared with that of the oxygen originally employed, shows the composition of the substances. The author then details some precautions necessary to be observed in

the process, and points out some of its peculiar advantages; the chief of which is, that it is not liable to be affected by moisture.

The composition of the saccharine principle is next considered, under which term he includes all those substances in which hydrogen and oxygen unite, in the proportion in which they form water. These are all alimentary, or capable of becoming so; and, as they are chiefly derived from the vegetable kingdom, the author considers them as peculiarly deserving the name of vegetable aliments. Sugar is first examined; of which he states that there are at least two distinct varieties, and probably many more (besides the sugar of milk). The most perfect form of this principle is sugar-candy prepared from cane-sugar, the composition of which he states to be, carbon 41·379, and water, 58·261; identical in composition with which are all the most pure specimens of the loaf-sugar of commerce. Sugar, in this state, contains water of crystallization; and the author states that he shall give his reasons at length for considering this principle, in the abstract, as consisting of carbon 44·44, and water 55·55.

The other variety of sugar, considered by the author as distinct, was obtained from Narbonne honey. This was beautifully white, and crystallized in spherules; deprived of its hygrometric water, it was found to consist of carbon 36·36, and water 63·63. Between these two extremes, sugars of almost every possible grade occur; probably, in many instances, from mixture of the above two varieties. Some analyses of other sugars are given, such as East India sugar-candy and refined sugar; the sugar from the maple, beet-root, &c.

The next class of bodies considered, is the amylaceous, and the author commences with some remarks on the sense in which he employs the term Protorganized. He states that he has satisfied himself, from many observations, that the minute quantities of foreign bodies found in all organic products, instead of being mechanically mixed with them, as usually supposed, perform the most important functions; in short, that organization would not exist without them; that when a crystallized substance passes into the organized state, its chemical composition frequently remains essentially the same, and that the only difference that can be traced in it is the presence of a little more or less of water, and invariably of minute portions of some of the foreign bodies above alluded to; and that these appear not only to destroy its power of crystallizing, but usually to change entirely its sensible properties. This subject he promises fully to illustrate hereafter, but proposes in the mean time to adopt the word Protorganized, to designate all those substances formed essentially on the principles of crystallized bodies, but not capable of assuming that form, probably on account of the presence of the foreign bodies above alluded to.

Starch from wheat, the author considers as the most perfect form of this principle; the carbon from which varies from 38 to 40 per cent., according to the degree to which it has been dried. Arrow-root, which may be considered as a *low* variety of starch, analogous to the low sugar of honey, contains still more water, capable of sepa-

ration, than wheat-starch. It is to the want of attention to these circumstances that the author assigns the different results given by chemists with respect to the composition of this principle; which in the abstract, or free from water, he considers as identical with cane-sugar similarly circumstanced.

The next principle considered is vinegar, a substance that in almost all ages and countries, either by accident or design, has been more or less used as an aliment. The author states that he had reason to suspect long ago that the hydrogen and oxygen in this acid existed in the proportions which form water, but that he was not completely able to satisfy himself on the point till he employed the present apparatus. He decided the point by means of the acetate of copper, which produced no change of bulk in the oxygen employed. He states this acid to consist of carbon 47·05, and water 52·95; results that very nearly agree with those of other chemists. This principle is not, however, in the protorganized state, except the acid found in almost all animal matters, and hitherto called the Lactic acid, be deserving of that appellation.

The last substance connected with this series is *lignin*, or the woody fibre, a principle subject to all the varieties of starch before mentioned. The author finds the composition of this principle, in the abstract, as containing carbon 57·14, water 52·86; and observes, that he is not acquainted with it in the crystallized state, but that he has no doubt of its existence. In proof of the alimentary qualities of this principle, he quotes the experiments of Professor Autenrieth, of Tubingen, who states that when wood is reduced to a minute state of division, and subjected to other processes, which he describes, it is capable of gelatinizing like starch when boiled in water, and of forming bread.

The sugar of milk is next considered. This, in its crystallized state, is composed of carbon 45·45, water 54·54. Gum-arabic, according to the author, is this substance in the protorganized state, and, like all analogous substances, combines with any proportion of water; and hence the different compositions assigned to it by different chemists.

As connected with this subject, the author next proceeds to consider the oxalic, citric, tartaric, and saccholactic acids, the composition of each of which is given; and concludes by observing, that he purposely refrains from all chemical observations, till the whole of the facts in his possession are laid before the Society.

Experiments to ascertain the Ratio of the Magnetic Forces acting on a Needle suspended horizontally, in Paris and in London. By Captain Edward Sabine, *of the Royal Artillery, Sec. R.S.* Read June 21, 1827. [*Phil. Trans.* 1828, *p.* 1.]

The needles used in these experiments were cylinders 0·16th of an inch in diameter, and 2·4 inches in length, pointed at the ends, and suspended by a silk fibre 5 inches long, over the centre of a graduated

ivory circle. The needle, previous to beginning to count the vibrations, was drawn 50° or 60° from the magnetic meridian by another needle, and left to oscillate. When it had reduced its arc of vibration to 30°, the counting of its vibrations was commenced, and terminated at 5°. It usually took between 300 and 400 vibrations to reduce the arc of vibration to this limit, occupying from 12 to 16 minutes. Four of the needles, with an apparatus in duplicate, were sent to the author from Professor Hansteen of Christiana, to be employed in comparative experiments in various parts of Great Britain. They were vibrated in Edinburgh by Captain Basil Hall and Lieut. Robert Craigie, and the results are set down with the rest in this paper. The needles being returned, were also used in the experiments between Paris and London. The two remaining needles were made by Dollond, of the same size and form as Professor Hansteen's. The author then relates his experiments, which were made on the 3rd of December, about seven weeks previous to his departure for Paris, in the garden of the Horticultural Society at Chiswick; and on the 15th of January, at Thornfold Park, near Tunbridge Wells; and on the 30th of January, in the garden of the Royal Observatory at Paris.

An opportunity occurring, three of the needles were sent to England early in April, and, with one sent by Captain Hall from Edinburgh, were vibrated by Captain Chapman, R.A., in the garden of the Horticultural Society, and returned to Paris.

Professor Hansteen's four needles were always kept separate, but those by Mr. Dollond together, and nearly in contact. To try the effect of separation, these were separated from the 14th of March to the 30th of April, and being then again tried in the same place as before, their times of vibration were found unaltered.

The author then states, in the form of an abstract, the results of the several experiments, all the details of which are subjoined in the form of tables. The mean of all gives a ratio of horizontal directive force in Paris greater than in London, in the ratio of 1068 : 1000; and on the supposition that the dip in London is 69° 45′, and in Paris 67° 58′, the ratio of the intensity of directive force on the dipping-needle comes out greater in London than in Paris, by about 15 parts in 1000.

On the Resistance of Fluids to Bodies passing through them. By James Walker, *Esq. F.R.S.E. Communicated by* Davies Gilbert, *Esq. M.P. V.P.R.S.* Read May 31, 1827. [*Phil. Trans.* 1828, *p.* 15.]

The object of this paper is to explain a new mode of measuring the resistance of fluids, which has of late become more than formerly an object of research owing to the introduction of steam navigation.

The resistance of a fluid *per se*, is, theoretically speaking, as the square of the velocity; but, independently of friction and viscidity, this theory is only applicable to the case of a body entirely and deeply immersed. If it float on the surface (as a boat), the elevation of the water in front, and its depression behind, disturbs the exactness of

this law, and renders theory inapplicable; while experiment, as the author remarks, has not yet supplied the defect.

The author devotes the first part of his paper to a consideration of the experiments made by Bossut and other members of the French Academy in 1776, 1778, and by the London Society for the Improvement of Naval Architecture in 1793 and 1798. Both these sets of experiments he regards as inconclusive, partly from the small size of the floating body and the small velocities used, but chiefly from the inadequacy of the means of measuring the actual resistance, and the sources of error arising from the rigidity of the cords used, the friction of pulleys, and that of the line itself dragged through the water, the moving power being a weight suspended and descending uniformly.

In the experiments which form the object of this paper, all these sources of error (which, in some cases, amounted to three times the resistance to be measured,) were avoided by the simple contrivance of estimating the strain exerted on the boat, at every instant, by a spring weighing machine immediately attached to it, through which the tension of the cord was of course transmitted, and which measured the actual tension exerted in overcoming the resistance of the boat, unmixed with any of the other causes of the destruction of power.

The apparatus employed is illustrated by drawings. The experiments were made in the East India Import Dock, whose size and depth are such as to allow no resistance arising from the sides or bottom of the dock. The boat being drawn at each experiment over $\frac{4}{10}$ths of a mile, the time of passing over $\frac{1}{10}$th was carefully noted, and the tension or resistance read off and registered every two seconds. The velocity was preserved uniform by applying the power of men turning a barrel in measured time by the swing of a pendulum.

Four sets of experiments were made on boats of 18 and 28 feet in length, variously loaded, and on a Thames wherry, with velocities from 2 to $5\frac{1}{2}$ miles per hour; and the conclusion from them all is, that the resistance increases in a higher ratio than as the square of the velocity.

Mr. Walker concludes this paper with a comparison between the effect of moving power applied on a rail-road and on a canal, which from these experiments appears to be reduced to equality at lower velocities than if the resistance to the boat were as the square of the velocity.

On the Corrections in the Elements of Delambre's *Solar Tables required by the Observations made at the Royal Observatory, Greenwich. By* George Biddell Airy, *Esq. M.A., Fellow of Trinity College, Cambridge, and Lucasian Professor of Mathematics in the University of Cambridge. Communicated by* John Frederick William Herschel, *Esq. V.P.R.S.* Read December 6, 1827. [*Phil. Trans.* 1828, *p.* 23.]

The author was desired by the Board of Longitude to examine the discordancies between the right ascensions of the sun, as observed at

Greenwich, since the erection of the new transit instrument, and as computed by the solar tables of Delambre, which are used in the computation of the Nautical Almanac; with a view to the discovery of the errors in the elements of those tables. The number of observations from which this comparison was made is 1212, and they extend, with an interruption of only three months, from the end of July 1816 to the end of the year 1826. The result of the comparison at first indicated the necessity of a correction of the epochs of the sun's longitude, and of the longitude of the perigee, and perhaps also of the equation of the centre. But upon pursuing the examination through a series of years, it became manifest that some other source of irregularity existed, and that this could be no other than an erroneous estimate of the masses of some of the planets, especially of Venus and of Mars. A more critical examination showed, that there was also an error in the assigned mass of the moon.

The author proceeds to state the process by which he arrived at the determination of the amount of these several corrections. It was found necessary in these investigations to take into account an error which occurred in the tables with regard to the secular motion. It results from his researches, that the epochs for 1816 and those for 1821 to 25 ought to be increased respectively by $4''\cdot734$ and $5''\cdot061$; that of the perigee increased by $46''\cdot3$, and the greatest equation of the centre diminished by $0''\cdot84$. The mass of Venus should be reduced in the proportion nearly of 9 to 8, and that of Mars nearly in the proportion of 22 to 15. On a comparison of these results with those which have been derived from an examination of some of Dr. Maskelyne's observations, as given by Burkhardt in the *Connaissance des Tems* for 1816, they are found on the whole to agree in the most satisfactory manner. The principal discordance occurs in the correction of the place of the perigee; a discordance which the author thinks may arise either from want of correctness in the calculation of the term in the motion of the perigee, depending on the square of the time, or, what is more probable, from some undiscovered inequality in the formula, which is a function of the sun's mean longitude.

Experiments to determine the Difference in the Length of the Seconds Pendulum in London and in Paris. By Captain Edward Sabine, *of the Royal Artillery, Secretary of the Royal Society. Communicated by* Thomas Young, *M.D., Foreign Secretary to the Royal Society, and Secretary to the Board of Longitude.* Read November 15, 1827. [*Phil. Trans.* 1828, *p.* 35.]

The author commences this paper by a brief statement of the existing state of the determinations of standards of length in the two countries; and he observes an attempt made by M. Arago in 1817 and 1818, to bring into immediate comparison the standards of the two countries, proved inconclusive from the rates of the pendulums not having been obtained with sufficient exactness.

The author having obtained from His Grace the Master-general of the Ordnance a general leave of absence from his military duties so long as he could be usefully employed in scientific pursuits, conceived he could no way satisfy the condition better than in carrying into effect this purpose. Accordingly, being provided with two pendulums, one made for M. Schumacher, another the property of the Board of Longitude, he set out for Paris, whither the pendulums were forwarded to him.

The comparison was made in Paris at the Royal Observatory, in the Salle de la Méridienne, on the spot in which M. Biot's measurement had been made, and every proper facility and assistance afforded him. The coincidence-clock was compared every 12^{h} by M. Mathieu with the transit-clock of the Observatory. On the 27th of April, the weather having set in mild and steady, the experiments were begun. The results are stated in the form of appended tables, of which a detailed account is given. Table I. contains the daily rate of the clock used for the coincidences. Table II. contains the particulars of thirteen distinct determinations of the rate of the pendulum No. 8.; four by M. Mathieu; four by M. Nicollet; three by the author; one by Messrs. Nicollet and Savary conjointly; and one by M. Savary and the author. They are corrected as usual. Table III. contains the results of thirteen similar determinations of the rate of the pendulum No. 7, in which the author was also assisted by Messrs. Freycinet and Duperrey.

Each of the pendulums, when not used in observing coincidences, was employed in determining its rate by a journeyman-clock or counter,—a method used by Messrs. Freycinet and Duperrey, but which the author thinks inferior to that of coincidences, though capable of giving good results. The particulars of these are given in Tables IV. and V. From all these experiments in conjunction, it appears that the numbers of vibrations performed in a mean solar day at Paris (reduced as usual) by the two pendulums, were respectively 85922·06 and 85933·83.

The pendulums and apparatus were re-conveyed to London early in September by water, and the rates again determined at Mr. Browne's house in Portland Place, by means of that gentleman's excellent clocks, and transit observations made by Captain Sabine. The precautions used are fully detailed, and the observations, which are also appended in a tabular form in Tables III, VII, VIII, IX, X, XI, the author being assisted by M. Quetelet, of Brussels. They give as a final result 85933·29 and 85945·85 for the numbers of vibrations respectively, made by each in a mean solar day, similarly reduced for London.

As a final result of the whole operation, the author regards $12^{v}\cdot00$ as the acceleration of the seconds pendulum in passing from Paris to London. The same acceleration deduced from a comparison of M. Biot's and Captain Kater's direct measurements of the seconds pendulum, in Paris and in London, comes out 11·76, or, conversely, the length of the seconds pendulum observed by the former in London

transferred to Paris by an assumed retardation of 12°, gives a length differing from M. Biot's by 0·00023. Borda's agrees within 0·00079 with M. Biot's, and Captain Kater's, so transferred, holds very nearly a mean between the two, but approaches rather nearer to Biot's than to Borda's.

On the Measurement of High Temperatures. By James Prinsep, *Esq. Assay Master of the Mint at Benares. Communicated by* Peter Mark Roget, *M.D. Secretary of the Royal Society*. Read December 13, 1827. [*Phil. Trans.* 1828, *p.* 79.]

The author, after adverting to the many abortive endeavours of former experimentalists to obtain instruments for the accurate admeasurement of high temperatures, and after suggesting doubts as to the confidence to which Wedgwood's pyrometer is entitled, describes several attempts of his own to effect this very desirable object. In the course of his inquiries, a remarkable fact presented itself to his notice in the change which occurred in an index constructed on the compensation principle, and formed by two slips of metal, the one of silver and the other of gold, originally quite pure, and united without any alloy. In the course of a few years, although it had never been subjected to a heat above that of melting lead, the whole surface of the gold became converted into an alloy of silver, the impregnation extending gradually to a considerable depth in the gold, and destroying the sensibility of the instrument to changes of temperature. After trying various plans, he gave the preference to one founded on the following principles: namely, that the fusing points of the pure metals are fixed and determined; that those of the three noble metals, namely, silver, gold, and platina, comprehend a very extensive range of temperature; and that between these three fixed points in the scale, as many intermediate ones as may be required may be obtained by alloying the three metals together in different proportions. When such a series of alloys has been once prepared, the heat of any furnace may be expressed by the alloy of least fusibility which it is capable of melting. The determinations afforded by a pyrometer of this kind will, independently of their precision, have the advantage of being identifiable at all times and in all countries: the smallness of the apparatus is an additional recommendation, nothing more being necessary than a little cupel, containing in separate cells the requisite number of pyrometric alloys, each of the size of a pin's head. The specimens melted in one experiment need only to be flattened under the hammer in order to be again ready for use. For the purpose of concisely registering the results, the author employs a simple decimal method of notation, which at once expresses the nature of the alloy, and its correspondence with the scale of temperature. Thus G .23 P would denote an alloy of gold with 23 per cent. of platina. As the distance between the points of fusion of silver and of gold is not considerable, the author divides this distance on the scale into ten degrees; obtaining measures of each by a suc-

cessive addition of 10 per cent. of gold to the silver, the fusion of which, when pure, marks the point of zero; while that of gold is reckoned at ten degrees. If minuter subdivisions were required for particular objects of research, these might easily be made, following always the decimal series. From the point of fusion of pure gold to that of pure platina, the author assumes 100 degrees, adding to the alloy which is to measure each in succession 1 per cent. of platina. Whether these hypothetical degrees represent equable increments of temperature is a question foreign to the purpose of this paper, and must be the subject of future investigation. The author then enters into a detailed account of the method he employed for insuring accuracy in the formation of the requisite series of alloys, and of various experiments undertaken to ascertain their fitness as measures of high temperatures. The determinations of the heats of the different furnaces adapted to particular objects, are given in a tabular form. The remaining portion of the paper contains the recital of the author's attempts to determine by means of an apparatus connected with an air thermometer, the relation which the fusing point of pure silver bears to the ordinary thermometric scale. An extensive series of experiments, of which the results are given in a table, were made with this apparatus. From the data thus afforded, after making the necessary corrections, the author deduces the following results in degrees of Fahrenheit: viz. A full red heat 1200°; orange heat 1650°; melting point of silver (which had been estimated by Wedgewood at 4717°, and by Daniell at 2233°,) 1830°; of silver alloyed with one tenth gold 1920°.

The paper is accompanied with drawings of the apparatus employed.

On Captain Parry's *and Lieutenant* Foster's *Experiments on the Velocity of Sound. By Dr.* Gerard Moll, *Professor of Natural Philosophy in the University of Utrecht. Communicated by Captain* Henry Kater, *V.P.R.S.* Read January 17, 1828. [*Phil. Trans.* 1828, *p.* 97.]

In this paper the author institutes a comparison between the results of the experiments on the velocity of sound made by Captain Parry and Lieutenant Foster in the arctic regions, and those deduced from the theoretical formula of Laplace. At the temperature of $-17°\cdot72$ of Fahrenheit, and with a barometric pressure of 29·936 inches, the mean result of all the observations gave a velocity of 1036·19 feet per second. With the same data, this velocity, by calculation according to the formula of Laplace, would be 1017·72; differing from the observed velocity by 17·47 feet only. Similar comparisons of particular observations, which are selected as being made under favourable circumstances, with the deductions from theory, are made by the author with but little variation in the results. From the whole investigation, he draws the conclusion, that in very high latitudes, where the cold is very intense, the data on which the

calculations are founded are more uncertain than at ordinary temperatures; thus confirming the opinion of Laplace, who says, in speaking of these differences, "qu'elles paraissent être dans les limites des petites erreurs dont cette expérience, et les élémens du calcul, dont j'ai fait usage, sont encore susceptibles." The author shows by an elaborate calculation, that the influence of moisture in the atmosphere on the velocity of sound must at these two temperatures be quite trifling, not amounting in any case to more than a few inches in a second; and that this element may therefore be safely neglected in the computation. He next proceeds to compare the experiments of the northern navigators with those of Dr. Van Beck and himself, and also those of other observers, adverting to their general agreement; which, he observes, is sufficiently satisfactory to warrant the conclusion, that whatever difference may still be found to exist between computation and observation, is rather to be ascribed to some imperfection in the theoretical formula, than to any fault or negligence in the observers. He concludes by remarking the strong testimony which Captain Parry's observations afford of his exemplary accuracy, amidst the great difficulties with which, from the circumstances in which he was placed, he had generally to contend.

An Account of a Series of Experiments made with a view to the Construction of an achromatic Telescope with a fluid concave Lens, instead of the usual Lens of Flint Glass. In a Letter addressed to Davies Gilbert, *Esq. M.P. President of the Royal Society. By* Peter Barlow, *Esq. F.R.S. &c.* Read January 17, 1828. [*Phil. Trans.* 1828, *p.* 105.]

The idea of constructing achromatic telescopes with fluid lenses was first suggested to the author by the attempt of Messrs. Gilbert to apply to practice the principles and rules for the construction of aplanatic object-glasses, laid down by Mr. Herschel in the Philosophical Transactions for 1821. In following these suggestions, the author became sensible of the difficulty of obtaining flint glass of sufficient size and purity for astronomical telescopes; and was thence led to consider the possibility of substituting some fluid in place of flint glass. Dr. Blair had, many years ago, succeeded in making very perfect telescopes of this description, but he still retained the use of flint glass. Among the various fluids adapted to this optical purpose, the author gave a decided preference to the sulphuret of carbon, which combines properties of perfect transparency and freedom from colour, with a refractive index nearly equal to that of flint glass, and with a dispersive power more than double, properties which it appears to retain under all the temperatures to which it is likely to be exposed in an astronomical telescope. After several trials, Mr. Barlow determined the best method of confining this fluid, but was at first unsuccessful in his attempts to construct with it a telescope of 6 inches aperture and 7 feet in length. He afterwards undertook a smaller one of 3 inches aperture, which he at length accomplished, and in the very first trials with it was able to separate a great num-

ber of double stars of the class which Sir William Herschel has pointed out as tests of a good $3\frac{1}{2}$-inch refractor. Encouraged by his success, he again attempted a 6-inch object-glass, with a different manner of adjusting and securing the lenses, and considers the result of his endeavour as proving at least the practicability of the construction. This instrument, with a power of 143, shows the small star in Polaris so distinct and brilliant, that its transit might be taken with the utmost certainty; it exhibits distinctly the small stars in α Lyræ, Aldebaran, Rigel, &c. and decidedly separates Castor, γ Leonis, and ϵ Bootis. The belts and double ring of Saturn are well exhibited with a power of 150; and the belts and satellites of Jupiter are tolerably defined with the same power, but will not bear a higher power than about 200.

In the usual construction of achromatic telescopes, the two or the three lenses composing the object-glass are brought into immediate contact. But the high dispersive power of the sulphuret of carbon enables Mr. Barlow to place the fluid correcting lens at a distance from the plate object lens equal to half its focal length. By this means the fluid lens, which is the most difficult part of the construction, is reduced to one half or less of the size of the plate lens. This construction, therefore, renders us independent of flint glass, enables us to increase the aperture of the telescope to a considerable extent; and gives us all the light, field, and focal power of a telescope of one and a half time the length of the tube. The author investigates analytically the formulæ for calculating the proper distance of the lenses on this construction, and expresses a hope that further experiments will enable us to determine the precise distance which shall reduce what has been termed the secondary spectrum, inseparable from the ordinary construction, either to zero, or to an inconsiderable amount.

A Catalogue of Nebulæ and clusters of Stars in the Southern Hemisphere, observed at Paramatta in New South Wales, by James Dunlop, *Esq. In a Letter addressed to Sir* Thomas Makdougall Brisbane, *Bart. K.C.B. late Governor of New South Wales. Presented to the Royal Society by* John Frederick William Herschel, *Esq. Vice President.* Read December 20, 1827. [*Phil. Trans.* 1828, *p.* 113.]

The observations, of which the results are here given, were made by Mr. Dunlop in the open air, with a 9-feet reflecting telescope, having the clear aperture of the large mirror 9 inches, and fitted up as a meridian telescope; the position of which, and the index error, being ascertained by the passage of known stars. The drawings which accompany the paper were made at the time of observation of the appearances of a great number of nebulæ and clusters, and particularly of the nebulæ major and minor. The paper contains a catalogue and description of 629 nebulæ, arranged in the order of their south polar distance, and in zones for each degree in the order of their right ascension. A few observations are subjoined, describing more particularly the appearance of the nebula minor, which,

seen through the telescope, resembles one of the brighter portions of the milky way; of the nebula major, which is brighter, more irregular, and composed of a great number of different parts; and of the dark space on the east side of the cross, or the black cloud, as it is called, which is occasioned by the almost total absence of stars. It is remarked by the author, that neither of the two nebulæ, major and minor, are at present in the place assigned to them by La Caille. He finds also that scarcely any nebulæ exist in a high state of condensation, and very few even in a state of moderate condensation towards the centre. Some have bright points in or near the centre, many of which may be stars; but the greater number of the nebulæ appear only as condensations of the general nebulous matter into faint nebulæ of various forms and magnitudes, generally not well defined; while many of the larger nebulous appearances are resolvable into stars of small magnitudes. But whether nebulæ are universally thus resolvable, is a question of which our instruments are yet incompetent to afford a direct solution, and in the discussion of which we have only analogy as our guide.

An Account of Trigonometrical Operations in the Years 1821, 1822, *and* 1823, *for determining the Difference of Longitude between the Royal Observatories of Paris and Greenwich. By Captain* Henry Kater, *V.P.R.S.* Read January 31, and February 7, 1828. [*Phil. Trans.* 1828, *p.* 153.]

The first section of this paper contains a narrative of the proceedings of the Commission appointed for executing the object announced in the title.

The first trigonometrical operations for connecting the meridians of Paris and Greenwich were carried on by General Roy, in cooperation with Messrs. de Cassini, Mechain, and Legendre, in the year 1790, an account of which was published in the Philosophical Transactions of that year. In 1821, the Royal Academy of Sciences, and Board of Longitude at Paris, communicated to the Royal Society of London their desire that these operations should be repeated, and the following Commissioners were nominated by these scientific bodies for that purpose; namely, Messrs. Arago and Mathieu, on the part of the Academy of Sciences; and Lieutenant-Colonel Colby and Captain Kater, on the part of the Royal Society.

The instrument employed in these operations was the great theodolite of Ramsden, belonging to the Royal Society, and the same which had formerly been used by General Roy.

It was at first proposed to adopt as a base, some one of the distances given by the trigonometrical survey of Great Britain, and to connect it with General Roy's stations; but it was found that the guns and wooden pipes, which had marked these stations, had been either removed or lost, so that the exact stations could not be immediately ascertained. The signals used for connecting the stations upon the coasts of England and France were lamps with compound lenses,

constructed under the direction of M. Fresnel. These lenses were 3 feet in diameter, and were composed of numerous pieces; the light they gave far exceeded that of any of our lighthouses, appearing at the distance of 48 miles, like a star of the first magnitude. Staffs were also erected near the lamps, but these were only occasionally visible.

These lamps were placed under the care of proper attendants on stations selected upon Fairlight Down and near Folkstone Turnpike, and the observers then crossed the Channel on the 24th of September 1821, and stationed themselves at Blancnez. Their observations were for some time retarded by tempestuous weather, but were completed on the 7th of October. They then removed to Montlambert, a fort situated on a height near Boulogne, where a further delay occurred in consequence of an accident that had happened to the lamp at Fairlight. On the 14th they returned through Calais to Fairlight. Here they at length succeeded in discovering the wooden pipe by which General Roy had marked his station, and which had been buried 4 feet deep in the earth. In order to preserve this point, a mill-stone, having the words "Roy's Station" cut upon it, was placed level with the surface of the ground, its centre being precisely over the centre of the pipe.

The party next proceeded to Folkstone; and in order to carry on the series towards London, stations were selected on Stede Hill and Wrotham Hill, which were connected with Folkstone by an intermediate point on Tolsford Hill, from which the stations on the French coast are visible, and the triangles connected with the church of Nôtre Dame at Calais. It was found impossible to connect the triangles directly with the base measured by General Roy upon Hounslow-heath, on account of many intervening buildings since erected, which now intercept the view of one end of the base from the other end. The summer of 1822 was employed in the choice of stations, one of which was the temporary meridian mark erected near Chingford for the Royal Observatory, and which was fixed upon in order that the side of one of the triangles might coincide with the meridian of Greenwich. Observations were next made from Leith Hill, Wrotham Hill, Stede Hill, Crowborough, and Severn-droog Castle, on Shooter's Hill, which were connected with Greenwich by the north-west pinnacle of Westminster Abbey and the cross of St. Paul's, and afterwards by means of Hanger Hill, with Fairlight Down and Folkstone turnpike. A difficulty occurred in observing from the station on Shooter's Hill the signal staff erected upon Hanger Hill, in consequence of the intervening smoke of London. This was obviated by a contrivance of Colonel Colby, consisting of several tin plates nailed to the staff, and disposed one above another at such angles as to reflect in succession the sun's rays in the direction of Shooter's Hill, and thus to answer the purpose of a heliostat. Each plate gave in its turn a neat image of the sun, resembling a fixed star, which was seen through a smoke so thick as to render the hill itself invisible.

In July of the next year, 1823, the theodolite belonging to the Ordnance was placed at the station at Chingford, and the series of observations, which had been left imperfect the preceding autumn, were completed. Pains were taken by sinking stones of proper size, with the word "station" marked upon them, and also by observing the angles formed by steeples and other permanent objects in the vicinity, to preserve the identity of the stations, for the use of future observers.

In the second section, an account is given of the methods of computation that were adopted. The methods which have been employed for geodetical calculations, may be reduced to three;—the first consists in considering the observed triangles as spherical triangles, of which the sides are to be computed by spherical trigonometry; the second is to deduce from the spherical angles, the angles formed by the chords, and thus reduce the spherical triangle to a plane triangle; the third consists in deducting from each of the observed spherical angles, one third of the spherical excess, which reduces the triangle to a plane triangle, in which the computations may be made by plane trigonometry. This last method, devised by Le Gendre, and which is recommended by its elegance and simplicity, was the one employed on the present occasion.

The third section comprises the details of the observations of the several triangles, and the computations made from them of the actual distances of the several stations. As the standard of linear measure employed by General Roy for the measurement of the base upon Hounslow Heath differs by a small fraction from the Imperial standard yard, it was found necessary to add 5·82 feet as a correction to that distance. Corresponding corrections to all General Roy's measurements may be obtained by using the constant multiplier of ·0000691.

The fourth section is occupied with computations of the distances from the meridian, and from the perpendicular to the meridian of Greenwich.

The fifth section contains the investigations of the latitudes and longitudes of the several stations. In these computations the ellipticity of the earth has been assumed at one 300dth, as being nearly the mean between $\frac{1}{310}$ and $\frac{1}{290}$, the limits between which the ellipticity is generally supposed to be comprised.

The sixth section contains remarks on the observations of the pole star for determining the direction of the meridian, showing the small degree of reliance to which, with reference to that object, such observations are entitled, and also on the determination of the length of the degree upon a circle perpendicular to the meridian, which is liable to similar causes of error.

The seventh section is on the measurement of the heights of the stations above the level of the sea; and the influence of terrestrial refraction on the accuracy of these measurements.

As the results of M. Arago's observations in France have not yet been published, the longitude of Calais must be taken, as given in the

Connaissance des Temps, to be 9° [illegible]' 55' west of Paris; which added to [illegible] as east longitude from Greenwich, resulting from the preceding work, gives 2° 20' [illegible] for the difference of longitude between Paris and Greenwich; which is equivalent to 9m 21s·18 in time, differing only [illegible] in defect from the results obtained with fire signals as reported in the Phil. Trans. for 1826, by Mr. Herschel. As the accuracy of the preceding work wholly depends upon the degree of reliance that may be placed upon the base at Hounslow Heath, which is somewhat questionable, the author recommends that a new base be measured to connect in the most unexceptionable manner the stations of Leith Hill and Wrotham; to the successful accomplishment of which, the arrangements so happily devised by Colonel Colby for compensating expansion would eminently contribute.

An Appendix is subjoined, containing miscellaneous observations with respect to various objects connected with the proceedings, accompanied with tables relating to the computations. The methods employed for securing the permanence of the positions of the microscopes of the theodolites are pointed out, and the influence of various minute causes of inaccuracy is inquired into, especially that of lateral refraction, which frequently occasioned a sensible variation in the same angles observed in different states of the weather.

The original observations relating to the work are deposited with the Royal Society, for the purpose of being consulted whenever occasion may require. All the angles employed in the work, with the name of the observer, and the manner in which they were derived, are given at the end of the present paper. Tables are also given detailing the observations of the pole star.

On the Phenomena of Volcanoes. By Sir Humphry Davy, *Bart. F.R.S.* Read March 20, 1828. [*Phil. Trans.* 1828, *p.* 241.]

In a paper on the Decomposition of the Earths, published in the Philosophical Transactions for 1812, the author offered it as a conjecture that the metals of the alkalies and earths might exist in the interior of the globe, and on being exposed to the action of air and water, give rise to volcanic fire and to the production of lavas; by the slow cooling of which, basaltic and other crystalline rocks might subsequently be formed. Vesuvius, from local circumstances, presents peculiar advantages for investigating the truth of this hypothesis; and of these, the author availed himself during his residence at Naples in the months of December 1819, and of January and February 1820. A small eruption had taken place a few days before he visited the mountain, and a stream of lava was then flowing with considerable activity from an aperture in the mountain a little below the crater, which was throwing up showers of red-hot stones every two or three minutes. On its issuing from the mountain, it was perfectly fluid, and nearly white-hot; its surface appeared to be in violent agitation, from the bursting of numerous bubbles, which emitted clouds of white smoke. There was no appearance of more vivid ignition

in the lava when it was raised and poured out by an iron ladle. A portion was thrown into a glass bottle, which was then closed with a ground stopper; and on examining the air in the bottle some time afterwards, it was found not to have lost any of its oxygen. Nitre thrown upon the surface of the lava did not produce such an increase of ignition as would have attended the presence of combustible matter. The gas disengaged from the lava, proved on examination to be common air. When the white vapours were condensed on a cold tin plate, the deposit was found to consist of very pure common salt; and the vapours themselves contained 9 per cent. of oxygen, the rest being azote, without any notable proportion of carbonic acid or sulphurous acid gases; although the fumes of this latter gas were exceedingly pungent in the smoke from the crater of the volcano. On another occasion the author examined the saline incrustations on the rocks near the ancient bocca of Vesuvius, and found them to consist principally of common salt, with some chloride of iron, a little sulphate of soda, a still smaller quantity of sulphate or muriate of potassa, and a minute portion of oxide of copper. In one instance in which the crystals had a purplish tint, a trace of muriate of cobalt was detected. From the observations made by the author at different periods, he concludes that the dense white smoke which rose in immense columns from the stream of lava, and which reflected the morning and evening light of the purest tints of red and orange, was produced by the salts which were sublimed with the steam. It presented a striking contrast to the black smoke, arising from the crater, which was loaded with earthy particles, and which in the night were highly luminous at the moment of the explosion. The phenomena observed by the author afford a sufficient refutation of all the ancient hypotheses, in which volcanic fires were ascribed to such chemical causes as the combustion of mineral coal or the action of sulphur upon iron, and are perfectly consistent with the supposition of their depending upon the oxidation of the metals of the earths upon an extensive scale in immense subterranean cavities, to which water, or atmospheric air, may occasionally have access. The subterranean thunder heard at great distances under Vesuvius, prior to an eruption, indicates the vast extent of these cavities; and the existence of a subterranean communication between the Solfaterra and Vesuvius is established by the fact, that whenever the latter is in an active state, the former is comparatively tranquil. In confirmation of these views, the author remarks that almost all the volcanoes of considerable magnitude in the old world are in the vicinity of the sea: and in those where the sea is more distant, as in the volcanoes of South America, the water may be supplied from great subterranean lakes; for Humboldt states that some of these throw up quantities of fish. The author acknowledges, however, that the hypothesis of the nucleus of the globe being composed of matter liquefied by heat, offers a still more simple solution of the phenomena of volcanic fires.

Abstract of a Meteorological Journal kept at Benares during the Years 1824, 1825, and 1826. By James Prinsep, *Esq. Assay Master of the Mint at Benares. Communicated by* Peter Mark Roget, *M.D. Secretary of the Royal Society.* Read January 25, 1828. [*Phil. Trans.* 1828, *p.* 251.]

The registers, of which the monthly results are presented in a tabular form, contain an account of the states of the barometer, thermometer, the air hygrometer, the wet bulb hygrometer, the degrees of aqueous tension, amount of evaporation, quantity of rain, wind, and other circumstances relating to the weather at Benares during three successive years. The author states it as his opinion, that the diurnal oscillations of the barometer, as well as the monthly variations, are dependent on the alterations of the specific gravity of the air from changes in its temperature; the operations of which may be traced with much greater distinctness at Benares than in the more variable climates of Europe. He illustrates his position by graphic representations of the changes in the barometer, corresponding with those of the thermometer.

He next points out a method of reducing the indications derived from hygrometers of different constructions, to absolute degrees of aqueous tension: and concludes, by expressing a hope that the cultivators of hygrometrical science in Europe would take pains to furnish standard instruments of comparison at the several principal stations on the Indian continent; as such a measure is the only security which can be obtained against inaccuracy in the observations that are now very extensively carried on in that part of the world.

A Description of a vertical floating Collimator; and an Account of its application to Astronomical Observations with a Circle and with a zenith Telescope. By Captain Henry Kater, *V.P.R.S.* Read April 24, and May 1, 1828. [*Phil. Trans.* 1828, *p.* 257.]

The construction of the instrument which forms the subject of this paper, is a material improvement on that of the horizontal floating collimator, of which an account was given by the author in the Philosophical Transactions for 1825. Its superiority is derived from its adaptation to the vertical instead of the horizontal position, by which the sources of error arising from the necessity of transferring the instrument to different sides of the observatory, and of taking the float out of the mercury and replacing it, at each observation, are wholly obviated. The vertical floating collimator has the further advantage of being adapted for use, not only with a circle, but also with a telescope, either of the refracting or reflecting kind. Such a telescope, furnished with a wire micrometer, and directed to the zenith, becomes a zenith telescope, free from all the objections to which the zenith sector, and the zenith telescope, with a plumb line, are liable.

The instrument itself is supported on a square mahogany stand, which slides on two parallel beams fixed at the upper part of the ob-

servatory in the direction of the meridian, and which has a circular aperture in the centre, having at its edge a projecting rim of iron to admit of the passage of the telescope. The telescope, of which the focal length is 8 inches, is supported in the vertical position by a bridge connecting it with a circular iron ring, 10 inches and $\frac{8}{10}$ths in diameter, which floats in mercury. The mercury is contained in a circular iron trough, the central aperture of which is sufficiently large to allow of its turning freely round the rim, which rises from the margin of the aperture of the stand. The object-glass of the telescope is placed at its lowest end, and its focus is occupied by a diaphragm, composed of two brass plates, each cut so as to form an angle of 135°, and placed opposite to each other, so that the angular points are brought to an accurate coincidence; thus leaving on each side intervening spaces, which form vertical angles of 45° each. The telescope below, whether belonging to a circle or a zenith telescope, is to be directed so that the image of these angles shall be bisected by the micrometer wire; for which purpose the diaphragm of the collimator is illuminated by a bull's-eye lantern, placed at a convenient distance upon one of the beams crossing the observatory, the light being reflected downwards by a plane mirror placed on a screen with a suitable aperture immediately above the collimator. The collimator is then to be turned half round in azimuth, the motion being facilitated by rollers, and limited, as to extent, by two catches which receive a projecting wire fixed to the outer circle of the trough. When in this situation, the observation of the diaphragm by the telescope, and the bisection of its angles, are to be repeated, and the mean of the two positions will indicate the exact point of the zenith.

Minute directions are given by the author for the construction of all the parts of the collimator, and for their proper adjustments; together with an account of the precautions to be taken in the employment of the instrument. The time required for completing the determination of the zenith point by its means, need not exceed two minutes; and if to this be added the time necessary for a second set of observations of the same kind, for the purpose of verification, and of a nearer approach to accuracy, the whole time required will not be more than five minutes, during which it is not probable that any sensible disturbance can have taken place in the position of the instrument from changes of temperature.

Tables are given containing registers of numerous series of experiments, made both by the author and by several of his friends, with a view to determine the stability of the instrument and the degree of reliance that can be placed in the results. In the first series, out of 60 independent determinations of the zenith point, there are 25, the error of each of which does not exceed $\frac{1}{10}$th of a second; 37 under $\frac{2}{10}$ths; 47 under $\frac{3}{10}$ths; 55 under $\frac{4}{10}$ths; 3 between 4- and 5-tenths; and 2 a little above half a second. But the author thinks it probable that the greater part of these errors, minute as they are, must be attributed to want of power in the micrometer; which power is directly as the focal length of the object-glass or mirror of the telescope to

[illegible] which necessarily limits the precision of [illegible].

[illegible] with a collimator [illegible] having a focus of only 5 inches in [illegible] the errors generally do [illegible] a second.

[illegible] the manner of using the vertical [illegible] observations, beginning with the [illegible] described by the Rev. F. Wol[illegible] applicable to other similar [illegible]. The new collimator affords also the most perfect me[illegible] circle or of placing [illegible].

The author [illegible] method of applying the [illegible]. In comparing the observations [illegible] belonging to the Board of Ordnance, with [illegible] with the vertical floating col[illegible] was $-0''\cdot54$ and $-0''\cdot75$; [illegible]. From observations made on γ Dra[illegible] is 0^s 2' 6"·36, and [illegible] the difference of latitude between the two [illegible] that of Greenwich being 51° [illegible]. The decimals of a se[illegible] and the horizontal floating [illegible] the same instrument and the vertical floating [illegible] and the vertical floating [illegible].

[illegible] the employment [illegible] construction, [illegible] by using it, the [illegible] long, the use [illegible] will be wholly [illegible].

On the Height of the Aurora Borealis above the surface of the Earth; particularly one seen on the 29th of March, 1826. By John Dalton, *F.R.S. Read April 17, 1828. [Phil. Trans. 1828, p.* 291.]

The author observes that opinions differ as to the elevation of the Aurora Borealis above the surface of the earth, and that this is a point which can be determined only by a series of concurring observations. The appearance of a phenomenon of this kind on the 29th of March, 1826, assuming the form of a regular arch at right angles to the magnetic meridian, and marked by peculiar features, continuing for above an hour in the same position, afforded a most favourable opportunity for obtaining the data requisite for the solution of this problem; and the author accordingly took great pains to collect as many authentic accounts as possible of the apparent position of this luminous arch with reference to the stars, when seen

from various places where it had been observed in England and in Scotland. It appears to have been actually seen in places 170 miles distant from one another, in a north and south direction, and 45 miles distant from east to west, thus comprising an area of 7000 or 8000 square miles; but it must have been visible over a much greater extent. Accounts were received of its having been seen as far north as Edinburgh, and as far south as Manchester and Doncaster, and at most of the intermediate towns; and from the exact correspondence of the descriptions from all these places, it was impossible to doubt that they referred to the same luminous appearance. In proceeding from north to south, the apparent altitude of the arch continually increased, still keeping to the south of the zenith till we come to Kendal, at which place it very nearly crossed the zenith; at Warrington, which is further south, the culminating point of the arch was north of the zenith. Wherever seen, the arch always seemed to terminate nearly in the magnetic, east and west, at two opposite points of the horizon.

The observations, in which the author places the greatest confidence for determining the height of this aurora, were those made at Whitehaven and at Warrington, places which are distant 83 miles from one another, and situated nearly on the same magnetic meridian. Calculating from the data they afford, he finds the height of the arch very nearly 100 miles above the surface of the earth, and immediately over the towns of Kendal and of Kirkby-Stephen. This conclusion is corroborated by observations at Jedburgh; but if the former be compared with those at Edinburgh, the height will come out to be 150 or 160 miles, and the position vertical about Carlisle: but he thinks the former result more entitled to confidence. Assuming the height to be 100 miles, it will follow that the breadth of the arch would be 8 or 9 miles, and its visible length in an east and west direction from any one place would be about 550 miles. The author then proceeds to take a comparative view of the results of inquiries on the height and position of other auroræ which have at different times appeared, and are recorded in the Philosophical Transactions and other scientific journals. He also gives an account of a luminous arch seen both at Kendal and at Manchester on the 27th of December last, which appeared in the zenith at the former place, and was elevated 53° from the north at the latter place; whence its height is deduced to be 100 miles. From the general agreement of this series of observations, the author infers that these luminous arches of the aurora, which are occasionally seen stretching from east to west, are all nearly of the same height; namely, about 100 miles. Observations are still wanting for the determination of the length of beams parallel to the dipping-needle, which constitute the more ordinary forms of the aurora borealis; neither can it be determined whether these beams arise above the arches, as from a base, or whether they descend below, as if appended to the arches. It is remarkable that the arches and beams are rarely, if ever, seen connected together, or in

juxta-position; but always in parts of the heavens at a considerable distance from each other.

A Comparison of the Changes of Magnetic Intensity throughout the Day in the Dipping and Horizontal Needles, at Treurenburgh Bay in Spitsbergen. By Captain Henry Foster, *R.N. F.R.S.* Read May 8, 1828. [*Phil. Trans.* 1828, *p.* 303.]

The observations made by the author at Port Bowen in 1825, on the diurnal changes of magnetic intensity taking place in the dipping- and horizontal-needles, appeared to indicate a rotatory motion of the polarizing axis of the earth, depending on the relative position of the sun, as the cause of these changes. By Capt. Foster's remaining at Spitzbergen, during the late Northern Voyage of Discovery, a favourable opportunity was afforded him of prosecuting this inquiry. Instead of making observations with a single needle, variously suspended, as had been done at Port Bowen, two were employed,—the one adjusted as a dipping-needle, and the other suspended horizontally. The relation between the simultaneous intensities of the two needles could thus be ascertained, and inferences deduced relative to the question whether a diurnal variation in the dip existed as one of the causes of the observed phenomena, or whether, the dip remaining constant, they were occasioned by a change in the intensity.

The dipping-needle used was one belonging to the Board of Longitude, and made by Dollond. Both this and the horizontal-needle were made in the form of parallelopipedons, each 6 inches long, 0·4 broad, and 0·05 thick. The experiments were continued from the 30th of July to the 9th of August; and were so arranged, that in the course of two days an observation was made every hour in the four-and-twenty; that is, part of them in one day and another part in the other day.

The observations on the horizontal-needle were made in the following manner:—after being freely suspended by a silk thread divested of torsion, the needle was turned somewhat more than 40° out of the magnetic meridian, and the oscillations counted only when the arc of vibration had decreased to 40°. The times of performing ten oscillations were then noted successively until 200 were completed; the terminal arc and the temperature of the instrument were also registered. The oscillations of the dipping-needle were taken as follows: one hundred with the face of the instrument east, previous to those of the horizontal-needle being observed; and another hundred after the latter, with the face west,—a process which gives the mean time of observation nearly the same for both needles. Two tables are given; the first containing a register of the observations; and the second, the mean proportional intensities at every hour in each needle, deduced from the respective times of the performance of 100 oscillations. From a comparison of the changes occurring in

the two needles, it appears that at the time when an increase took place in the intensity of the dipping-needle, that of the horizontal-needle underwent a corresponding diminution, and *vice versâ*. On comparing these results with the hypothesis of a rotation of the general polarizing axis of the earth about its mean position as a centre, and employing for this investigation the formulæ given by Mr. Barlow, in his Essay on Magnetic Attractions, it is found that the radius of this circle of rotation is very nearly eight minutes. The magnitude of this radius, however, will be considerably influenced by the sun's declination.

The change of intensity of the dipping-needle, in as far as it is owing to a variation of the dip, would be only in the proportion of 3726 to 3732; whereas its actual amount is found to be one eighty-third part of the whole. This, therefore, seems to imply changes in the general magnetic intensity of the earth. But the author, limiting his present inquiry to the variations in the dip, concludes that the times of the day when these changes are the greatest and the least, are such as indicate a constant inflexion of the magnetic pole towards the sun during the diurnal rotation, and to point to the sun as the primary agent in the production of these changes.

Experiments relative to the Effect of Temperature on the refractive Index and dispersive Power of Expansible Fluids, and on the Influence of these Changes in a Telescope with a fluid Lens. By Peter Barlow, *Esq. F.R.S. &c.* Read May 15, 1828. [*Phil. Trans.* 1828, *p.* 313.]

In a paper lately read to the Society, the author stated that he had not perceived any change in the focal length of the telescope, induced by changes of temperature; but he has since ascertained that in order to produce the brightest and most perfect image, the distance of the object-glass requires a minute adjustment, amounting to 0·134 of an inch, corresponding to an elevation of temperature from 57° to 84°, or a depression from 57° to 31°.

In order to introduce greater clearness and precision, the author proceeds to define certain terms which he finds it necessary to employ. By the *length of the telescope*, he would be understood to mean the distance between the object-glass and the focus; by the *fluid focus*, that between the fluid lens and the focus; and by the *focal power* of the telescope, he means the focal length of a telescope of the usual construction, which gives the same convergency to the rays, or produces an image of the same size: but he also employs the term *focal length of the telescope*, as synonymous with the first; that of *fluid focal length* as synonymous with the second; and that of *equivalent focal length* as synonymous with the last of these terms.

As it is difficult to determine the refractive index of the fluid under different circumstances, from which their effects on the focal power of the telescope might be computed, the author endeavoured to ascertain, by direct observations, the effect of changes of temperature

on the power of the telescope, and thence computes the corresponding change in the refractive index of the fluid. The result is the amount of adjustment already stated. The correction for angular measurements was the 60th part of a second in every minute, for every degree of thermometric change; a quantity, Mr. Barlow observes, which is too small to deserve notice, except in cases of extreme delicacy. The dispersions at 31° and at 84° are in the ratio of 3067 to 3084. The change in the refractive index between 32° and 212°, supposing it to increase uniformly, would be about one tenth of the whole,—a proportion which is very nearly the same as the actual expansion of the fluid. Hence it is considered as probable, that in this and all other expansible fluids, the index of refraction varies directly as the density. On the other hand it would appear, that the dispersive ratio remains, at all temperatures, constantly the same.

On some Circumstances relating to the Economy of Bees. By Thomas Andrew Knight, *Esq. F.R.S. President of the Horticultural Society.* Read May 22, 1828. [*Phil. Trans.* 1828, *p.* 319.]

The author had already stated, in a former communication to the Royal Society, his having noticed that for several days previous to the settling of a swarm of bees in the cavity of a hollow tree adapted to their reception, a considerable number of these insects were incessantly employed in examining the state of the tree, and particularly of every dead knot above the cavity which appeared likely to admit water. He has since had an opportunity of observing that the bees who performed this task of inspection, instead of being the same individuals as he had formerly supposed, were in fact a continual succession of different bees; the whole number in the course of three days being such as to warrant the inference that not a single labouring bee ever emigrates in a swarm without having seen its proposed future habitation. He finds that the same applies not only to the place of permanent settlement, but also to that where the bees rest temporarily, soon after swarming, in order to collect their numbers.

The swarms, which were the subjects of Mr. Knight's experiments, showed a remarkable disposition to unite under the same queen. On one occasion a swarm, which had arisen from one of his hives, settled upon a bush at a distance of about twenty-five yards; but instead of collecting together into a compact mass, as they usually do, they remained thinly dispersed for nearly half an hour; after which, as if tired of waiting, they singly, one after the other, and not in obedience to any signal, arose and returned home. The next morning a swarm issued from a neighbouring hive, and proceeded to the same bush upon which the other bees had settled on the preceding day; collecting themselves into a mass, as they usually do when their queen is present. In a few minutes afterwards a very large assemblage of bees rushed from the hive from which the former

swarm had issued, and proceeded directly to the one which had just settled, and instantly united with them. The author is led from these and other facts to conclude that such unions of swarms are generally, if not always, the result of previous concert and arrangement.

The author proceeds to mention some circumstances which induce him to believe that sex is not given to the eggs of birds, or to the spawn of fishes or insects, at any very early period of their growth. Female ducks, kept apart from any male bird till the period of laying eggs approached, when a musk drake was put into company with them, produced a numerous offspring, six out of seven of which proved to be males.

The mule fishes found in many rivers where the common trout abounds, and where a solitary salmon is present, are uniformly of the male sex; hence the spawn must have been without sex at the time it was deposited by the female.

The author states that he has also met with analogous circumstances in the vegetable world, respecting the sexes of the blossoms of monoecious plants. When the heat is excessive, compared with the quantity of light which the plant receives, only male flowers appear; but if the light be in excess, female flowers alone are produced.

On the Laws of the Deviation of Magnetized Needles towards Iron. *By* Samuel Hunter Christie, *Esq. M.A. F.R.S. &c.* Read June 5, 1828. [*Phil. Trans.* 1828, *p.* 325.]

The author had pointed out, several years ago, the law of deviation of a magnetized needle, (either freely suspended or constrained to move in any particular plane,) from its natural position, by the influence of masses of iron in its vicinity. This law was founded on the hypothesis that the iron *attracted* both the poles of the needle: the position of which, resulting from this action, might be determined by that of an imaginary minute magnetic needle, freely suspended by its centre of gravity, reduced to the plane of revolution. The author had considered this law as fully established from its accordance with experiment; but Mr. Barlow, in a paper which was published in the last volume of the Philosophical Transactions, denies that such an accordance exists, and infers, from the results of some experiments which he made on horizontal needles, having their magnetism unequally distributed in their two branches, that the theory on which the preceding law is founded is fallacious. In opposition to the views of Mr. Barlow, the author contends that the phenomena observed are precisely those which must result from the theory he had himself adopted; and that they tend in no way to support the hypothesis of their being simply the effects of the magnetic power which the iron receives by induction from the earth.

The author was also led to suspect the accuracy of another conclusion which had been drawn by Mr. Barlow, namely, that the

length of the needle had no sensible influence upon the extent of its deviations. In order to determine this point, he began by ascertaining more scrupulously than had yet been done, the values of several of the elements of the calculation, such as the exact positions of the points where the intensity of the magnetism is the greatest, and also of the point of neutrality, or of the magnetic centre; and he next subjected to a more severe scrutiny a law which had been regarded as established by experiment; namely, that the tangent of the deviation is proportional to the rectangle of the cosine of the longitude, into the sine of the double latitude of the position of the centre of the needle, with relation to the mass of iron as referred to a hollow sphere.

In the course of his experiments the author ascertained that if any bar of steel, uniformly magnetized by the method of double touch, have this state of magnetism disturbed by drawing the end of a magnet from its centre to the end, having the same polarity as that applied to it, then the pole at that end will be shifted towards the centre, while the opposite pole will be removed further from it, and a corresponding change will occur in the position of the magnetic centre. Changes will also take place in the absolute intensities of the magnetism at each pole. Considerable differences were observed in the extent of the deviations of a needle six inches in length, and of one of two inches long, when successively placed in the same position with relation to the shell of iron. At the distance of 16·8 inches they amounted to more than two degrees and a half; and the difference continued to be very sensible even at a distance of 24 inches from the shell. In general when the needles were near to the north or south of the centre of the shell, the deviations of the longer needle exceeded those of the shorter; and the reverse took place when the needles were placed on the east and west sides. Hence he concludes that the efficacy of a small mass of iron placed near to the needle to serve as a compensation to the effects of more distant masses, will depend upon its being itself at such a distance from the needle as that the difference of its action upon a long and a short needle shall be insensible.

The author proceeds to deduce from the law which he has proposed various forms of equations for determining the deviations of a horizontal-needle due to the action of an iron sphere or shell applicable to different circumstances and conditions of the case. In one set of equations, actions and resulting positions are referred to three rectangular co-ordinates proceeding from the centre of the needle; and in another set they are referred to polar co-ordinates relative to the vertical and to the plane of the horizon. He next deduces equations for computing the deviations of a needle in which the magnetism has been disturbed by applying to one of its poles the corresponding pole of a magnet. He then proceeds to the detail of experiments for investigating the above-mentioned laws, and to their comparison with the results deduced from theory. These experiments appear to him to establish, beyond all doubt, the influence

which the length of a needle has on its deviations, produced by the attraction of the shell of iron. When examined by the test of the formulæ given by the author, the law of the tangent of the deviation being proportional to the rectangle of the cosine of the longitude into the sine of the double latitude on which so much dependance had been placed, is found to give results so inconsistent with one another, that it cannot be considered as even affording an approximation to the truth, and must therefore be wholly rejected.

The close agreement which the author found between the observed and the computed deviations of needles, whose magnetism had been disturbed by contact with a magnet, as well as those which had suffered no disturbance, fully confirmed the author in the views which he originally took of the action of iron on magnetized needles. He conceives that his hypothesis, instead of being at variance with observation, is not only consistent with all the experiments that have been made, but by affording the proper corrections tò be applied to them, derives the strongest support from these observations.

He concludes by mentioning a fact which he conceives to be irreconcileable with the hypothesis of induced magnetism; namely, that a steel bar, rendered as hard as it was possible to make it, produced, when its ends are reversed, precisely the same effect on the needle as a bar of the softest iron under similar circumstances.

Description of a Sounding Board in Attercliffe Church, invented by the Rev. John Blackburn, *Minister of Attercliffe-cum-Darnall, Sheffield.* Read June 5, 1828. [*Phil. Trans.* 1828, *p.* 361.]

The church at Attercliffe had long been remarkable for the difficulty and indistinctness with which a voice from the pulpit was heard. These defects have been completely remedied by the erection of a concave sounding-board, having the form resulting from half a revolution of one branch of a parabola on its axis. It is made of pine wood; its axis is inclined forwards to the plane of the floor, at an angle of about 10° or 15°; it is elevated so that the speaker's mouth may be in the focus; and a small curvilinear portion is removed on each side, so that the view of the preacher from the side galleries may not be intercepted. A curtain is suspended from the lower edge for about 18 inches on each side.

The effect of this sounding-board has been to increase the volume of sound to nearly five times what it was before; so that the voice is now audible, with perfect distinctness, even in the remotest parts of the church, and more especially in those places which are situated in the prolongation of the axis of the paraboloid. But the side galleries are also benefited; probably from the increase of the secondary vibrations in a lateral direction. Several experiments are related illustrative of these effects; among which, the most striking, was one in which a person placed so as to have one ear in the focus of the paraboloid, and the other towards another person speaking

from the remote end of the church, heard the voice in a direction the reverse of that from which it really proceeded.

The superior distinctness of sounds proceeding from the focus is accounted for by their all arriving at the same moment of time at a plane perpendicular to the axis, after reflexion from the surface of the paraboloid; which is a consequence of the equality of the paths they have described.

On the mutual Action of Sulphuric Acid and Alcohol, and on the Nature of the Process by which Ether is formed. By Henry Hennell, *Esq. Communicated by* William Thomas Brande, *Esq. F.R.S.* Read June 19, 1828. [*Phil. Trans.* 1828, *p.* 365.]

The most abundant product resulting from the mutual action of sulphuric acid and alcohol, without the application of heat, is the sulphovinic acid; but on distillation this peculiar product disappears, and ether is formed; and it becomes a question what part the sulphovinic acid plays in this process. In opposition to the assertion of Messrs. Dumas and Boullay, that this acid is not concerned in the production of ether, the author contends that whenever ether is formed, it is in consequence of the decomposition of the sulphovinic acid. He obtained ether from this latter fluid by distillation, when neither sulphuric acid nor alcohol were present; but if a certain quantity of water has been previously added, the sulphovinic acid is resolved into alcohol and sulphuric acid, and no ether is obtained; whereas during the distillation of ether in the ordinary way, the sulphovinic acid is re-converted, more or less, entirely into sulphuric acid. Hence he infers that the formation of the sulphovinic acid is a necessary and intermediate step to the production of ether from alcohol and sulphuric acid. As ether may be formed from alcohol, by the intermedium of sulphuric acid, so by the same intermedium may alcohol be obtained from ether,—the sulphovinic acid being in either case formed according to the mode of combination of the hydrocarbonous base. This theory is also illustrated by the employment of olefiant gas as the hydrocarbonous base, for by combining this gas with sulphuric acid, we may form sulphovinic acid, from which we may obtain at pleasure, by varying the circumstances of the decomposition, either alcohol or ether.

Experiments and Observations on Electric Conduction. By William Ritchie, *A.M. F.R.S. Rector of Tain Academy.* Read June 19, 1828. [*Phil. Trans.* 1828, *p.* 373.]

According to the modern theory of electricity, metallic bodies, far from attracting the electric fluid, as is commonly believed, are of all bodies those which have the least attraction for that fluid, and being the best conductors for it, are entirely passive during its transit through them. In confirmation of these views, the author describes

experiments in which the electric spark was found to have penetrated through the side of a glass globe blown to an extreme degree of thinness. An electric jar, from which the air had been partially exhausted, could not be made to receive as high a charge as when the contained air was of the usual density, and when entirely exhausted could not be charged in any sensible degree; when filled with condensed air on the other hand, it retained a higher charge than before. The heated and consequently rarefied air surrounding a red-hot iron rod is found to conduct electricity with great facility. The same property is observed in the flame from a blowpipe, which may be regarded as a hollow cone containing highly rarefied air; as also, in a larger scale, in that of a volcano. Sir H. Davy had concluded from his experiments on voltaic electricity, that the conducting powers of metals are diminished by heat; but Mr. Ritchie infers from several experiments which bear more directly upon the question, that the metals afford no exception to the general law, that in all bodies heat increases the conducting powers; and explains the apparent anomaly in Sir H. Davy's experiments, by the dissipation of the electricity by the rarefied air surrounding the heated metals, which were used as conductors. He concludes his paper by describing an experiment which appears to establish, in respect to this law, a striking analogy between the electric and magnetic influences.

On Magnetic Influence in the Solar Rays. By Samuel Hunter Christie, *Esq. M.A. F.R.S. &c.* Read June 19, 1828. [*Phil. Trans.* 1828, *p.* 379.]

From the experiments described by the author in a former paper, it appeared that a magnetized needle vibrated under exposure to the sun's rays, came to rest sooner than when screened from their influence; that a similar effect was produced on a needle of glass or of copper, but that the effect on the magnetized needle greatly exceeded that upon either of the others. In the prosecution of this inquiry, the author has endeavoured to vary the experiments so as to obviate several causes of inaccuracy which might tend to invalidate the general conclusions he had before drawn. His first object was to compare the effects of the solar rays on an unmagnetized steel needle with one that was magnetized under the same circumstances; and the result was, that the latter was influenced in a more considerable degree than the former; and a similar difference was observed when the vibrations of a magnetized needle were compared with those of a needle made of glass or of copper. He ascertained that the diminution of the terminal arc of vibration, on exposure to the sun, was not occasioned merely by the heat imparted to the needles or surrounding medium, although this cause appeared in some instances to measure the intensity of the action which produced the diminution. In order to determine the comparative influence of the separate rays, he allowed them to fall on the needles after transmission through differently coloured fluids and glasses; but owing to

want of opportunity, he was obliged to abandon the inquiry before arriving at any determinate results: though as far as they went, they appeared to confirm the conclusion that the effects were dependent on the degree of light, and not on that of the heat. The red rays, however, appeared to have a greater effect in diminishing the terminal arc than the blue. In order to determine the single effect of temperature, independently of light, the needles were vibrated in close vessels surrounded with water of different temperatures; the results showed that the terminal arc was increased in air of higher temperatures, which is the reverse of what takes place from the direct influence of the solar rays; and that this effect, instead of being different in the magnetized and in the other needles, was nearly the same in all, of whatever materials they consisted, and whether magnetized or not. The author next endeavoured to ascertain the effects produced on the axes of vibration by the action of a common fire; these, though much less in degree, he found to be similar in kind to those of the sun.

The Bakerian Lecture. On a Method of rendering Platina malleable. By William Hyde Wollaston, *M.D. F.R.S. &c.* Read November 20, 1828. [*Phil. Trans.* 1829, *p.* 1.]

In this paper the author details the processes which, from long experience in the treatment of platina, he regards as the most effectual for rendering that metal perfectly malleable. When it is purified by solution in aqua regia, and precipitation with sal-ammoniac, sufficient care is seldom taken to avoid dissolving the iridium contained in the ore by due dilution of the solvent. The author states the exact degree of dilution requisite for this purpose, and the exact proportions in which the two acids are to be used. The digestion should be continued for three or four days, with a heat which ought gradually to be raised; and the fine pulverulent ore of iridium allowed to subside completely before the sal-ammoniac is added. The yellow precipitate thus obtained, after being well washed and pressed, must be heated with the utmost caution, so as to expel the sal-ammoniac, but at the same time to produce as little cohesion as possible among the particles of platina. It is then to be reduced to powder, first by rubbing between the hands, and next by grinding the coarser parts in a wooden mortar with a wooden pestle; because the friction of any harder substance would, by producing burnished surfaces, render them incapable of being welded together by heat. The whole is then to be well washed in clean water.

In this process the mechanical diffusion through water is made to answer the same purposes as liquefaction by heat in the case of the other metals; the earthy impurities being carried to the surface by their superior lightness, and the effect of fluxes being accomplished by the solvent powers of water.

The grey precipitate of platina being thus obtained in the form of an uniform mud or pulp, is now ready for casting, which is effected

by compression in a mould, formed of a brass barrel, six inches and a half long, and turned rather taper within, so as to facilitate the extraction of the ingot when formed. The platinum is first subjected to a partial compression by the hand with a wooden plug, so as to expel the greater part of the water. It is then placed horizontally in an iron press, of which a figure is given, constructed so as to afford great mechanical advantage to the power applied to produce compression. The cake of platina is then to be heated to redness by a charcoal fire, in order to drive off all the remaining moisture; afterwards subjected to the most intense heat of a wind furnace; and lastly struck, with certain precautions, while hot, with a heavy hammer, so as effectually to close the metal. The ingot thus obtained may, like that of any other metal, be reduced by the processes of heating and forging to any other form that may be required. It may then be flattened into leaf, drawn into wire, or submitted to any of the processes of which the most ductile metals are capable.

The perfection of the above method of giving complete malleability to platina, is proved by comparing the specific gravity of a fine wire of that metal obtained by this process, which is found to be 21·5, with that of a similar wire drawn from a button which had been completely fused by the late Dr. Clarke with an oxy-hydrogen blowpipe, and which the author ascertained was only 21·16. A further proof of the excellence of the method employed by the author is derived from the great tenacity of the platina thus obtained, as determined by a comparison of the weights required to break wires made of this metal, so prepared, and similar wires of gold and of iron. These weights he found to be in the proportion of the numbers 59, 50, and 60 respectively.

An account is subjoined of the process employed by the author for obtaining malleable palladium by the intermedium of sulphur; and also of that for procuring the oxide of osmium, in a pure, white, and crystallized state.

A Description of a Microscopic Doublet. By William Hyde Wollaston, *M.D. F.R.S. &c.* Read November 27, 1828. [*Phil. Trans.* 1829, *p.* 9.]

The author, considering that in all microscopes distinct vision is impeded instead of being assisted by whatever light may be thrown upon the object beyond what is fully commanded by the object-glass, obviates this evil by collecting the admitted light to a focus in the same plane as the object to be examined. For this purpose he employs a plane mirror to direct the light, and a plano-convex lens to collect it, the plane side of the lens being towards the object to be illuminated. Availing himself of the property possessed by that form of eye-piece for astronomical telescopes, called the Huygenian, of correcting both chromatic and spherical aberration, the author conceived that by applying to a microscope the same combination reversed, he might obtain similar advantages. The construction he

employs resembles two thimbles, fitted one within the other by screwing, and with a perforation at the extremity of each. In these perforations are fixed two suitable plano-convex lenses, which may thus have their axes easily brought into the same line by means of their plane surfaces; while their distance from each other may be adjusted by screwing, so as to produce the best effect of which they are capable. The best relative proportion of the foci of the two lenses, appears, from the trials made by the author, to be that of three to one. The distance between their plane surfaces should, in general, be about 1·4 of the shorter focus, but should be varied by trial, till the utmost possible degree of distinctness has been attained. The lenses must be fixed in their cells with their plane sides next to the object to be viewed. The exterior cell of the compound magnifier should be formed with a flanch, so that it may rest upon the piece that receives it. The plano-convex lens, by which the object is illuminated, is inclosed in a tube about six inches long, blackened in the inside, and having a circular perforation below of about three tenths of an inch in diameter, for limiting the lights reflected from the plane mirror. The centre of this aperture must be in the common axis of the lenses; and the image of the perforation formed by the large lens, must be brought, by proper adjustment of the distance of that lens, into the same plane as the object to be examined. With a microscope so constructed, the author has seen the finest striæ and serratures upon the scales of the Lepisma and Podura, and the scales upon a Gnat's wing, with a degree of delicate perspicuity not attainable with any other microscope he has tried. In consequence of the plane surface of the lens being next to the object viewed, the microscope of Dr. Wollaston possesses the important advantage of having its action undisturbed by the contact of a fluid under examination.

An Account of some Experiments on the Torpedo. By Sir Humphry Davy, *Bart. F.R.S.* Read November 20, 1828. [*Phil. Trans.* 1829, *p.* 15.]

The author, after noticing the peculiarities discovered by Walsh in the electricity of the Torpedo, and the opinion of Cavendish, that it resembles the action of an electrical battery weakly charged, adverts to the conjecture of Volta, who considered it as similar to that of the galvanic pile. Being on the coast of the Mediterranean in 1814 and 1815, the author, desirous of ascertaining the justness of Volta's comparison, passed the shocks given by living torpedos through the interrupted circuit made by silver wire through water, but could not perceive the slightest decomposition of that fluid; the same shocks made to pass through a fine silver wire, less than one thousandth of an inch in diameter, did not produce ignition. Volta, to whom the author communicated the results of these experiments, considered the conditions of the organs of the torpedo to be best represented by a pile, of which the fluid substance was a very imper-

fect conductor, such as honey, and which, though it communicated weak shocks, yet did not decompose water.

The author also ascertained that the electrical shocks of the torpedo, even when powerful, produced no sensible effect on an extremely delicate magnetic electrometer. He explains these negative results by supposing that the motion of the electricity in the torpedinal organ is in no measurable time, and wants that continuity of current requisite for the production of magnetic effects.

On a Method of comparing the Light of the Sun with that of the fixed Stars. By William Hyde Wollaston, *M.D. F.R.S.* Read December 11, 1828. [*Phil. Trans.* 1829, *p.* 19.]

In the Philosophical Transactions for the year 1767, a suggestion is thrown out by Mr. Michell, that a comparison between the light received from the sun and any of the fixed stars, might furnish data for estimating their relative distances; but no such direct comparison had been attempted. Dr. Wollaston was led to infer from some observations that he made in the year 1799, that the direct light of the sun is about one million times more intense than that of the full moon, and therefore very many million times greater than that of all the fixed stars taken collectively. In order to compare the light of the sun with that of a star, he took, as an intermediate object of comparison, the light of a candle reflected from a small bulb, about a quarter of an inch in diameter, filled with quicksilver, and seen, by one eye, through a lens of two inches focus, at the same time that the star or the sun's image, placed at a proper distance, was viewed by the other eye through a telescope. The mean of various trials seemed to show that the light of Sirius is equal to that of the sun seen in a glass bulb one tenth of an inch in diameter, at the distance of 210 feet, or that they are in the proportion of one to ten thousand millions; but as nearly one half of the light is lost by reflection, the real proportion between the light from Sirius and the sun is not greater than that of one to twenty thousand millions. If the annual parallax of Sirius be half a second, corresponding to a distance of 525,481 times that of the sun from the earth, its diameter would be 3·7 times that of the sun, and its light 13·8 times as great. The distance at which the sun would require to be viewed, so that its brightness might be only equal to that of Sirius, would be 141,421 times its present distance; and if still in the ecliptic, its annual parallax in longitude would be nearly 3″; but if situated at the same angular distance from the ecliptic as Sirius is, it would have an annual parallax, in latitude, of 1″·8.

On the Water of the Mediterranean. By William Hyde Wollaston, *M.D. F.R.S.* Read December 18, 1828. [*Phil. Trans.* 1828, *p.* 29.]

The late Dr. Marcet in his examination of sea-water, of which he has given an account in the Philosophical Transactions for 1819, had

been unable, for want of a sufficient number of specimens of water taken at various depths in the Mediterranean, to draw any certain inference as to what becomes of the vast amount of salt brought into that sea by the constant current which sets in from the Atlantic through the Straits of Gibraltar, and which, on the evaporation of the water, must either remain in the basin of the Mediterranean, or escape by some hitherto unexplained means. In the hope of obtaining further evidence on this question, he had requested Captain Smyth, R.N., who was engaged in a Survey of that sea, to procure specimens of water from the greatest accessible depths. The specimens collected by Captain Smyth were, in consequence of Dr. Marcet's death, given to other persons, and applied to other objects. Dr. Wollaston, however, fortunately obtained the three remaining bottles of the collection.

The contents of one of these, taken up at about fifty miles within the Straits, and from a depth of 670 fathoms, was found to have a density exceeding that of distilled water by more than four times the usual excess; and accordingly, it left upon evaporation more than four times the usual quantity of saline residuum. The result of the examination of this specimen accords completely with the anticipation, that a counter current of denser water might exist at great depths in the neighbourhood of the Straits, capable of carrying westward into the Atlantic as much salt as enters into the Mediterranean with the eastward current near the surface. If the two currents were of equal breadth and depth, the velocity of the lower current need only be one fourth of that of the upper current, in order to prevent any increase of saltness in the Mediterranean.

An Account of the preliminary Experiments and ultimate Construction of a Refracting Telescope of 7·8 *inches aperture, with a fluid concave Lens. In a Letter addressed to* Davies Gilbert, *Esq. P.R.S. By* Peter Barlow, *Esq. F.R.S. &c.* Read December 18, 1828. [*Phil. Trans.* 1829, *p.* 33.]

The author gives an account of the continuation of his experiments on the construction of refracting telescopes with fluid lenses, which the aid furnished him by the Board of Longitude enabled him to pursue. The instrument he particularly describes has a clear aperture of 7·8 inches, which exceeds by about an inch that of the largest refracting telescope in this country. The whole length of the tube, with the eye-piece, is 12 feet, but its effective focus is 18 feet. It carries a power of 700 on the closest double stars in South's and Herschel's catalogue, and shows them round and defined. This telescope is mounted on a revolving stand, which works with considerable accuracy as an azimuth and altitude instrument. The weight of the stand is about 400 pounds, and that of the telescope 130 pounds, being purposely made heavy in order to obtain steadiness; yet its motions are so smooth, and the power so arranged, as to be easily manageable by one person; and the star may be followed by a slight

touch of the apparatus for regulating the more delicate movements of the telescope.

In order to protect it from the weather, which was found to injure its action and derange its adjustments, the author erected an observatory to contain it, consisting of a light piece of carpentry, 16 feet in diameter, with a revolving conical roof rising 9 feet above the walls, containing about 360 square feet of surface, and weighing about 10 cwt. It is moveable by a simple apparatus, made to revolve and open to any required azimuth, by the application of a force of about 10 or 12 pounds.

His first object in the preliminary experiments was to ascertain the best position of the lenses for diminishing as much as possible the secondary spectrum. For this purpose he reverts to the formulæ given in a preceding paper, whence he deduces equations applicable to this object. The mode of constructing different parts of the telescope is then particularly described, especially that of the fluid lenses, and of effecting the proper centering, and other adjustments. He then describes its power when applied to several double stars. By its means η Persei, marked as a treble star in South's and Herschel's catalogue, is seen distinctly sextuple; four of the smaller of these stars, together with a larger one, form a miniature representation of Jupiter and his satellites. Of the planets, he has only had opportunities of trying the telescope on Venus, Saturn, and Mars, all of which appear with remarkable brightness and distinctness. The moon is also remarkably beautiful; every minute distinction of figure and shade being brought into view.—The paper concludes with a detailed description of the various parts of the telescope and stand, illustrated by a drawing.

On the Dip of the Magnetic Needle in London, in August, 1828. *By Captain* Edward Sabine, *of the Royal Artillery, Sec. R.S.* Read January 8, 1829. [*Phil. Trans.* 1829, *p.* 47.]

This paper commences by noticing that the Philosophical Transactions contain the record of observations on the dip of the needle in London, from the early part of the last century to the present time. That these observations all concur in showing a progressive decrease of the dip during the whole period in question, but that they are insufficient in number and frequency, and the earlier ones particularly, in the required accuracy to enable us to determine whether the annual decrease has been uniform or otherwise.

The author having taken much pains to obtain a correct determination of the dip in the Regent's Park, in August 1821 (published in the Philosophical Transactions for 1822), repeated his observations in August 1828, at the expiration of seven years from the former determination; an interval which he considered sufficient to throw light on the rate at which the dip is at present diminishing. In consequence of the increase of buildings in the Regent's Park, he was induced to change the place of observation to the Horticultural So-

Society's garden at Chiswick: the distance apart is about five miles, but the direction is as nearly as possible that of the line of equal dip.

The apparatus, modes of observing, and needles employed, are fully described. The needles were four in number; one, of the ordinary construction; a second, fitted with Professor Meyer's apparatus for avoiding the errors arising from the non-coincidence of the centres of gravity and motion; a third, having a cross of wires attached to the axis, on the well-known plan of Dr. Mitchell; and a fourth, devised by Mr. Dollond, the middle of which is a cube perforated at right angles, so that the axis may be inserted in eight different ways.

In addition to his own apparatus and needles, the author obtained from the Colonial Department the use of a smaller apparatus, with a needle on Professor Meyer's plan, the same which was used by Captain Franklin on his last land expedition. The observations with this apparatus were made by Mr. David Douglas, of the Horticultural Society. The results were as follows:—

With the ordinary needle	69° 46'·1
With Meyer's needle	69 47 ·4
With a needle having an adjusted axis ..	69 38 ·3
With Mr. Dollond's needle	69 51 ·7
With the smaller apparatus	69 51 ·4
Dip in London, in August, 1828	69 47 ·0

From the observations of 1821 and 1828, the author finds a decrease in the dip in London, of 17'·5 in seven years, or an annual decrease of 2'·5.

The average annual decrease for the century preceding 1821 appears, from the most authentic observations, to have exceeded 3'. On examining the series of observations made on the dip in Paris since 1798, by MM. Humboldt, Gay Lussac, and Arago, the author finds a corresponding indication of a recent diminution in the yearly decrease of the dip; it appearing, by those observations, that the average yearly decrease, in the first half of the period between 1798 and 1828, exceeded 4'·75; and in the second half fell short of 3'. He concludes by remarking, that a repetition of the observations in London, at the expiration of another seven years, and a continuation of those at Paris, will probably afford a decisive indication on this point; and notices, in case the annual change shall prove to be diminishing in this part of the world, the importance of determining the precise period at which the dip shall become stationary, and the minimum to which it shall then have arrived.

Remarks on the tendency to Calculous Diseases; with Observations on the Nature of urinary Concretions, and an Analysis of a large Part of the Collection belonging to the Norfolk and Norwich Hospital. By John Yelloly, *M.D. F.R.S. &c.* Read June 19, 1828. [*Phil. Trans.* 1829, *p.* 55.]

The account given by the author of his examination of the urinary calculi contained in the Norwich Collection, the total number of which is 649, relates more particularly to those which have been either purposely divided, or accidentally broken in the extraction, and which amount all together to about 330. He gives a tabular view of the results of his analyses of these calculi, and states, in the order of their occurrence from the centre, the consecutive deposits of the different materials of which they are composed. About one half of the specimens consist only of one description of substance, and the remainder are formed of alternating layers, more or less numerous, of most of the substances which enter into the composition of human urinary calculi. The distinction between the lithic acid and lithate of ammonia, though generally recognised abroad, was scarcely attended to in this country, until noticed by Dr. Prout. The lithic calculi form, as is usual, the most numerous class of concretions in the Norwich collection, where they amount to nearly a third of the whole number; and if the number of those containing either lithic acid, or lithate of ammonia, as a nucleus, be taken into account, it will appear, as already observed by Dr. Prout, that not less than two thirds of all urinary calculi either consist of the lithates, or have those substances as their nuclei; whence it may be inferred, that a large proportion of them probably owe their existence to the previous formation of such a nucleus. The deposition of the phosphates is not followed by that of the other materials. The oxalate of lime is the only substance entering into the composition of urinary calculi, which is ever found in the form of distinct and specific crystallization, and it then forms what is called the Mulberry calculus. The author is led from his observations to suspect that carbonate of lime, although rarely found in a separate form in calculi, is not an unfrequent concomitant of phosphate of lime: with the assistance of Dr. Prout and Mr. Faraday, he ascertained the presence of carbonate of lime in some of the specimens which were not previously supposed to contain it. This result was also confirmed by the analysis of several specimens of calculi from the collection in the Hunterian Museum, and also from the Museum of Guy's Hospital, which he was permitted to examine.

The author is in hopes of being able to make some additions to this communication, if he can obtain permission to divide some of the remaining calculi in the Norwich Collection, so as to give to the Society the result of the whole analysis.

Experiments to determine the Difference in the Number of Vibrations made by an Invariable Pendulum in the Royal Observatory at Greenwich, and in the House in London in which Captain Kater's *Experiments were made. By Captain* Edward Sabine, *of the Royal Artillery, Sec. R.S. Communicated by the President and Council.* Read December 11, 1828. [*Phil. Trans.* 1829, *p.* 83.]

The experiments of which an account is given in this paper, were made in compliance with a request of the Council of the Royal Society, made in December 1827, that Captain Sabine would ascertain the difference in the number of vibrations of a pendulum at Mr. Browne's house, and at the Greenwich Observatory. The author gives a description of the instruments used in the observations; the first series of which were made in Mr. Browne's house, from the 17th to the 20th of March inclusive, and gave as the mean result, 85963·60 vibrations in a mean solar day. A reduction is here introduced, derived from some experiments made on the difference which takes place in the times of vibration in vacuo and in air; the number of vibrations in the former case being, under the same circumstances as in the observations, 9·97 *per diem* less than in the latter. A corresponding series made at Greenwich in May, gave as the mean 85964·17 vibrations, thus indicating an acceleration of 0·57 vibrations *per diem*; but the difference of latitude and of height between the two stations would have led us from theory to expect a total retardation of 0·38 vibration in the same time. From a second set of observations at Greenwich, the diurnal acceleration appeared to be 0·52 vibration. Taking the mean of this and the former result, it appears that the total amount of the discordance between theory and experiment is 0·91 vibration *per diem*. The stations are conveniently situated for verifying the existence of this anomaly, and its magnitude is such as to preclude all uncertainty as to its existence. With regard to its cause, the author is confirmed in the opinions he formerly entertained on this subject.

Tables are subjoined, containing accounts of the rate of the clocks used at both stations, and of the particulars of each series of observations.

On a definite Arrangement, and Order of the Appearance and Progress, of the Aurora Borealis; and on its Height above the Surface of the Earth. In a Letter to Davies Gilbert, *Esq. M.P. P.R.S. By the Rev.* James Farquharson, *Minister of the Parish of Allford, Aberdeenshire.* Read January 22, and February 29, 1829. [*Phil. Trans.* 1829, *p.* 103.]

The results of the numerous observations of the author on the Aurora Borealis, which on several occasions were made under very favourable circumstances, had already been announced in a short paper, published in 1823 in the Edinburgh Philosophical Journal; and it was concluded from them that the Aurora Borealis has in all cases a determinate arrangement and figure, and follows an invariable

order in its appearance and progress; that the pencils of rays or streamers, as they are called, generally make their first appearance in the north; and as they rise from the horizon, assume the form of an arch, extending from east to west, and having its vertex in the plane of the magnetic meridian, the arch itself being at right angles to that plane. While the arch itself is near the horizon, its breadth from north to south is considerable; and the streamers of which it is composed appear to be nearly at right angles to the general line of the arch, their directions converging to a point a few degrees to the south of the zenith. As the arch moves forwards towards the south, its lateral dimensions appear to contract, the intensity of its light increases, and the directions of the streamers, still tending to the same point in the heavens, approach more nearly to parallelism with that of the arch. When it has passed the zenith, and arrived at the above-mentioned point, a little to the south of the zenith, the arch is seen as a narrow belt, 3° or 4° only in breadth, and with well-defined edges. In its further progress southwards, it again enlarges in breadth, and exhibits, in a reverse order, the same succession of changes as before. Hence, the author concludes that the streamers have individually a position nearly vertical or parallel to the magnetic dip; that they form a thin fringe, stretching often to a great distance from east to west, at right angles to the magnetic meridian; and that the movement of the fringe from north to south takes place by the extinction of streamers at its northern side, and the formation of new ones contiguous to its southern side.

From a variety of observations which are detailed in this paper, the author infers, in opposition to the opinion of Mr. Dalton, that the region occupied by this meteor is above, but contiguous to, that of the clouds, or at least to that in which aqueous vapour is condensed, so as afterwards to appear in the form of clouds. The height of this region he estimates as in general about 2000 feet above the surface; and he is of opinion, that while such is the height of the lower ends of the vertical streamers, their upper ends may have an elevation of 2000 or 3000 feet more.

Observations on the Functions of the Intestinal Canal and Liver of the human Fœtus. By Robert Lee, *M.D., Physician to the British Lying-in-Hospital. Communicated by Dr.* Prout, *F.R.S.* Read June 19, 1828. [*Phil. Trans.* 1829, *p.* 121.]

From the circumstances of the early development of the liver and intestines in the fœtus, of the copious supply of blood which they receive, and of the great space which they occupy in the abdomen, the author was led to the conclusion that they performed some important functions in the fœtal economy. Although no nutritive matter can be furnished by the mouth, yet the contents of different portions of the alimentary canal were found, both in appearance and chemical composition, to bear a striking analogy to those of the same portions of the canal in the adult, where the processes of assimilation and ab-

sorption are performed. A semi-fluid matter, possessing all the characters of albumen, is found closely adhering to the inner coats of the small intestine; and is more especially abundant around the papillary projection, through which the common duct of the liver opens into the duodenum, and diminishes in quantity as we trace it towards the termination of the ileum. The great intestines are generally distended with a dark green homogeneous fluid, containing no albumen, and apparently excrementitious. No albumen can be detected in the contents of the stomach. Hence the author infers that an absorption of some nutritious substance, which he brings forward several arguments to show must be derived from the liver, takes place from the intestinal canal in the latter months of gestation. He states that in two instances he detected the presence of a substance, similar to that which he had found in the duodenum, in the hepatic duct itself; hence he is led to the conclusion that the function of the liver in the fœtus is not confined to the separation of excrementitious matter from the blood, but that it supplies materials subservient to nutrition. That the substances existing in the intestines of the fœtus are not derived from the mouth, is proved by their being equally found in acephalous children, or where the œsophagus is impervious, as where no such mal-conformation exists.

A note is subjoined to this paper by Dr. Prout, giving an account of the mode by which he ascertained the chemical character of the substance referred to his examination; and the paper is accompanied by drawings of the intestinal tube in the fœtus.

Experiments on the Modulus of Torsion. By Benjamin Bevan, *Esq. Communicated by the President.* Read December 18, 1828. [*Phil. Trans.* 1829, *p.* 127.]

The object of the author in this paper is to ascertain the modulus of torsion in different species of wood, and also of metals, deduced from experiments on a large scale, which he conceives will furnish many useful data, applicable to practice by the mechanic and engineer. Care was taken that the specimens of wood which were the subjects of experiment were sound and dry, and free from any large knots; and their correct dimensions were ascertained by an improved kind of callipers.

To every specimen two indexes were attached; one, a few inches from the end, fixed in the clamp or vice, and the other, at a small distance from the attachment of the lever, to which the straining power was applied; and the length of the bar subjected to torsion was estimated by the distance of the points of attachment of the indexes. A pivot was fixed at the supported end of the bar, in lieu of its axis.

The author gives the following rule for finding the deflection of a prismatic shaft; namely, that it is equal to the product of the straining power into the square of the radius by which it acts, and into the length of the shaft, divided by the modulus of torsion into the fourth power of the side of the square shaft. He then gives a table of the

modulus of torsion in different woods, which he finds to vary from about 9000 to 30,000 pounds, and to follow nearly the order of the specific gravity. In the metals, the modulus of torsion is one sixteenth of the modulus of elasticity.

On a Differential Barometer. By the late William Hyde Wollaston, *M.D. F.R.S. Communicated by* Henry Warburton, *Esq. F.R.S.* Read February 5, 1829. [*Phil. Trans.* 1829, *p.* 133.]

The instrument described in this paper is capable of measuring with considerable accuracy extremely small differences of barometric pressure. It was originally contrived with the view of determining the force of ascent of heated air in chimneys of different kinds; but as its construction admits of any assignable degree of sensibility being given to it, it is susceptible of application to many other purposes of more extensive utility. A glass tube, of which the internal diameter is at least a quarter of an inch, being bent in the middle into the form of an inverted siphon, with the legs parallel to each other, is cemented at each of its open extremities into the bottom of a separate cistern about two inches in diameter. One of these cisterns is closed on all sides, excepting where a small horizontal pipe opens from it laterally at its upper part; while the other cistern remains open. The lower portion of the glass tube is filled with water, or other fluid, to the height of two or three inches; while the remaining parts of the tube, together with the cistern, to the depth of about half an inch, are filled with oil; care being taken to bring the surfaces of water in both legs to the same level, by equalizing the pressures of the incumbent columns of oil. If the horizontal pipe be applied to the key-hole of a door, or any similar perforation in a partition, between portions of the atmosphere in which the pressures are unequal, the fluid in the corresponding half of the instrument will be depressed, while it is raised in the opposite one, until the excess of weight in the column thus elevated will just balance the external force resulting from the inequality of atmospheric pressures upon the surfaces of oil in both cisterns. This excess, however, is equal only to the difference between the weight of the column of water pressing on one side, and that of an equal column of oil which occupies the same length of tube on the other side. This difference depending upon the relative specific gravities of the two fluids will, in the case of olive oil and water, be about one eleventh of the weight of the column of water elevated; but the sensibility of the instrument might be increased at pleasure, by mixing with the water a greater or less quantity of alcohol, by which the excess of its specific gravity over that of oil may be reduced to one twentieth, one thirtieth, or any other assignable proportion. The instrument may be converted into an anemometer by closing both the cisterns, and by applying to the upper part of each a trumpet-mouthed aperture opening laterally.

Some Observations relating to the Function of Digestion. *By* A. P. W. Philip, *M.D. F.R.S. L. & E.* Read January 16, 1829. [*Phil. Trans.* 1829, *p.* 137.]

The author, referring to his former papers published in the Philosophical Transactions, concludes that digestion requires for its due performance, both a proper supply of gastric secretion, and a certain muscular action in the stomach; the latter circumstance being required for the expulsion of that portion of food which has been acted upon by the gastric juice. Nervous power is necessary for secretion; but the muscular action of the stomach being excited by the mechanical stimulus of the contents of that organ, is independent of the nervous power. It has already been shown by the author, that after the removal of a portion of the eighth pair of nerves, the galvanic influence directed through these nerves will restore the secretion of gastric juice; but Messrs. Breschet and Milne Edwards have lately endeavoured to prove, that the same effect results also from mechanical irritation of the lower portions of the divided nerves. The author points out several circumstances which appear to have been overlooked by these gentlemen, and which, he thinks, invalidate the conclusions they have deduced from their experiments. He states that a certain quantity of digested food will always be found in the stomach of the animal for five or six hours after the operation, and even after the lapse of ten or twelve hours, from its being less completely changed, and therefore expelled more slowly than in the natural state. The paper concludes with the recital of experiments made for the author by Mr. Cutler, in which the contents of the stomach of a rabbit, whose eighth pair of nerves, after excision, had been kept mechanically irritated, were compared with those of another rabbit, in which the nerves had not been irritated, and of a third, which had been left undisturbed. All those who witnessed the result of this experiment, among whom was Mr. Brodie, were convinced that the irritation of the nerves had no effect whatever in promoting the digestion of the food; neither did it at all contribute to relieve the difficulty of breathing, consequent upon the section of the nerves.

Experiments on the Friction and Abrasion of the Surfaces of Solids. *By* George Rennie, *Esq. F.R.S.* Read June 12, 1828. [*Phil. Trans.* 1829, *p.* 143.]

The first part of this paper is occupied by a rapid review of the labours of mechanicians on the subject of friction, from the period of those of Amontons at the end of the 17th century, to those of Coulomb and of Vince in the years 1779 and 1784; from which the author draws the conclusion that the progress of knowledge in this department of science has been slow and unsatisfactory, and that a wide field is still left open to experimental investigation. With a view to elucidate several points not yet sufficiently ascertained by

former writers, the author instituted several sets of experiments; some calculated to determine the forces required for dragging bodies of various kinds along a horizontal surface, and others for measuring the angle at which a plane was required to be inclined to the horizon in order to admit of the body sliding down it, attention being paid to the circumstances of pressure, extent of surface, time of previous contact, and velocity of motion.

The following are the principal conclusions which the author deduces from his experiments. The friction of ice rubbing upon ice diminishes with an increase of weight; but without observing any regular law of increase. When dry leather is made to move along a plate of cast iron, the resistance is but little influenced by the extent of surface. With fibrous substances, such as cloth, the friction diminishes by an increase of pressure, but is greatly increased by the surfaces remaining for a certain time in contact; it is greater, *cæteris paribus*, with fine, than with coarse cloths; the resistance is also much increased by an increase of surface. With regard to the friction of different woods against each other, great diversity and irregularity prevail in the results obtained; in general the soft woods give more resistance than the hard woods: thus, yellow deal affords the greatest, and red teak the least friction. The friction of different metals also varies principally according to their respective hardness; the soft metals producing greater friction under similar circumstances than those which are hard. Within the limits of abrasion, however, the amount of friction is nearly the same in all the metals, and may in general be estimated at one sixth of the pressure. The power which unguents have in diminishing friction, varies according to the kind of the fluidity of the particular unguent employed, and to the pressure applied.

The paper is accompanied with drawings of the apparatus used; and the details of the experiments are given at length in a tabular form.

An Attempt to rectify the Inaccuracy of some Logarithmic Formulæ. *By* John Thomas Graves, *of the Inner Temple, Esq. Communicated by* John Frederick William Herschel, *Esq. V.P.* Read December 18, 1828. [*Phil. Trans.* 1829, *p.* 171.]

The discovery made by Poisson and Poinsot during their recent researches on angular sections, of errors in trigonometrical formulæ usually deemed complete, drew the attention of the author to an analogous incorrectness in logarithmic series. He accordingly proposes in the present paper to exhibit in an amended form two fundamental developements; the principles employed in the establishment of which admit of application in expanding by different methods various similar functions, and tend also to elucidate other parts of the exponential theory.

He then enters into an analytical investigation of the equation $a^x = y$, and exhibits correct developements; first, of y in terms of

a and x; and secondly, of x in terms of a and y; the corresponding developements hitherto given being incomplete. He considers the principles employed in this inquiry as presenting a solution of many difficulties, and illustrating peculiarities appertaining to the theory of logarithms of negative quantities; and when applied to geometry, as furnishing the means of tracing the form and developing the properties of curves whose equations involve exponential quantities. He also states that by their means various differential and other formulæ usually exhibited in treatises on logarithms may be rendered complete. An appendix is subjoined containing several examples of these applications of his principles. In the course of his investigations, the author endeavours to explain the remarkable anomaly which frequently presents itself to the analyst, of developements, in which, upon substituting a particular value for the variable in each, there is no approximation to numerical identity between the several resulting series, calculated to any number of terms, and the respective functions which they ought to represent. He combats the paradoxical opinion which has been advanced, that equations, which in particular instances are numerically false, are yet analytically true; and explains the difficulty by reverting to the limitations inherent in the hypothesis upon which the developement is founded. He maintains, in opposition to the opinions of Jean Bernouilli and D'Alembert, that the logarithms of negative and positive numbers are not in general the same; and hence infers that negative numbers have occasionally even real logarithms. The chief novelty of his system consists in showing that any assigned quantity, relatively to a given base, has an infinite number of orders of logarithms, and an infinite number of logarithms in each order.

On the Reflection and Decomposition of Light at the separating Surfaces of Media of the same and of different refractive Powers. By David Brewster, *LL.D. F.R.S. L. & E.* Read February 12, 1829. [*Phil. Trans.* 1829, *p.* 187.]

When white light is incident upon a surface which separates two different media, the portion that is reflected should, according to the Newtonian theory of light, preserve its whiteness, provided the thickness of either of the media exceed the eighty millionth of an inch. But since the dispersive powers of bodies are different, it must follow as a necessary consequence, that reflected light can never under any circumstances retain perfect whiteness, although the modification it experiences is not of sufficient amount to become sensible in ordinary experiments. The author during his investigations of the laws of polarization for light reflected at the separating surface of different media, had occasion to inclose oil of cassia between two prisms of flint glass, and was surprised to find that the light reflected was of a blue colour. The fact was new, but might be readily explained upon the principle that although the refractive density of oil of cassia greatly exceeds that of flint glass for the mean rays, yet the action

of these two bodies is nearly the same on the less refrangible rays: hence it may happen that a larger proportion of the former than of the latter is transmitted, and the pencil formed by reflexion will then appear blue. The partial decomposition thus effected in the incident rays will be the same in kind, though it may vary in degree, at different angles of incidence, and cannot therefore give rise to any variation of colour in the reflected rays, although they may differ in intensity according to the obliquity of the incidence. By using different kinds of glass, and of interposed fluids, the author obtained various analogous results, different rays of the spectrum being separated according to the prevalence, in each particular case, of one or other of the opposite actions exerted upon them by the solid and the fluid medium. The author directed his attention more particularly to those conditions in which the nearest approach could be made to a perfect equilibrium of all the forces which affect the incident rays. The solids which he employed in his experiments were two prisms of plate glass, of which the sections were right-angled isosceles triangles, and differing but very slightly in their refractive indices. The fluids were castor oil and balsam of copivi, the former having a less, and the latter a greater refractive power than the glass prisms; a thin film of either fluid being interposed between them. With castor oil, and within the limit of total reflexion, the reflected light is yellow; on gradually diminishing the angle of incidence, it passes in succession through all the tints of three orders of colours, of which the details are presented in a table exhibiting those which correspond to different angles of incidence. When the incident light is homogeneous, no colours are seen, but the reflected pencils have their maxima and minima of intensity; like the rays of thin plates, or the fringes of inflected light formed by homogeneous rays. When copivi balsam is employed as the fluid medium, the same orders of colours are obtained by reflexion, but at smaller angles of incidence than with castor oil.

Having ascertained that at a temperature of about 94° the mean refractive index of the balsam became equal to that of the glass prisms, the author examined the influence of a gradual elevation of temperature upon the colours of the reflected pencils; and found that no particular change marked the instant when the refractive densities of the two media became equal; although when the temperature was increased considerably, the tints entirely disappeared. Analogous results were obtained by employing prisms of obsidian instead of glass.

The author next engaged in more extensive series of experiments with various fluids interposed between glass prisms; and states their results in the form of a table, showing more especially the periods of colours produced at the separating surfaces by the different kinds of oils. He considers the facts which are there detailed, as establishing the existence of reflecting forces at the confines of media of the same refracting power; and as proving, first, that the reflective and refractive forces in these media do not follow the same law: and, secondly,

that the force which produces reflexion, varies according to a different law in different bodies. The reflective forces of the solid and the fluid may be conceived to decrease in various ways: first, they may respectively extend to different distances from the reflecting surface, and decrease according to the same law. Secondly, they may extend to different distances, and vary according to a different law; or, lastly, they may extend to the same distance, and vary according to different laws. Whether the refracting forces follow the same law in solids and in fluids, it is extremely difficult to determine by direct experiment; but if we assume the mutual dependence of the refracting and reflecting forces, then the experiments recorded in this paper will establish a variation in the law of the refracting forces of different media.

These facts may be explained on the undulatory theory of light, by supposing that the density or elasticity of the ether varies near the surface of different bodies, an hypothesis which has already afforded an explanation of the loss of part of an undulation in several of the phenomena of interference; the part lost being, according to Dr. Young, a variable fraction, depending on the nature of the contiguous media.

The phenomena of periodical colours at the confines of media of the same or of different refractive powers, are evidently dependent on the law of interference, although it may be difficult to point out the precise mode in which they are produced. In combinations where there is much uncompensated refraction, their production is influenced by certain changes, such as the formation of a thin and invisible film on the surface of the solid, the nature and origin of which the author endeavours to investigate, but which he acknowledges he has hitherto been unable to discover. That some unrecognised physical principle is the cause of all these phenomena will, he thinks, appear still more probable from a paper which he intends to present to the Society, on the production of the very same periods of colour, at similar angles of incidence, by the surfaces of metals and transparent solids, when acting singly upon light. He also announces, as the subjects of two other communications, the results of researches in which he has been long engaged; first, on the action of light on the surfaces of bodies as an universal mineralogical character, with the description of a lithoscope for discriminating minerals; and secondly, on the influence of the doubly refracting forces upon the ordinary forces, which reflect and polarize light at the surfaces of bodies.

On the Reduction to a Vacuum of the Vibrations of an Invariable Pendulum. By Captain Edward Sabine, *of the Royal Artillery, Sec. R.S. Communicated by Dr.* Thomas Young, *Secretary of the late Board of Longitude.* Read March 12 *and* 19, 1829. [*Phil. Trans.* 1829, *p.* 207.]

The experiments contained in this paper originated in a notice published by M. Bessel in the *Astronomische Nachrichten*, for

January 1828, announcing that he had found the theory usually employed for reducing the vibrations of a pendulum in air to the corresponding vibrations *in vacuo*, was incorrect, inasmuch as it omitted the expenditure of a portion of the moving force on the particles of the air which are set in motion by the pendulum in its vibration. In order to ascertain by the most direct mode of experiment the retardation which a pendulum experiences by vibrating in the medium of the atmosphere, the author constructed, at the expense of the late Board of Longitude, an apparatus in which an invariable pendulum could be vibrated alternately in air of the full atmospheric pressure, and in rarefied air approaching nearly to a vacuum. The apparatus was set up and employed in a room assigned for that purpose in the Royal Observatory at Greenwich: its description is given in this paper with reference to plates, and those processes are particularly dwelt upon which were ultimately successful in ensuring the immobility of the suspension of the pendulum, and in rendering the apparatus impermeable when the air was withdrawn from the interior. The arrangements for observing the coincidences of the pendulum with a clock, and for ascertaining the exact pressure of the air, both in its ordinary and rarefied state, are minutely described. When the air was either partially or wholly withdrawn, a correction was found to be required for the indications of the thermometer giving the temperature of the pendulum, to compensate the removal of the pressure of the atmosphere on the exterior of the ball and tube of the thermometer. The value of this correction for different states of exhaustion was ascertained by placing the thermometer in pounded ice under the receiver of an air-pump, and noting the height of the mercury corresponding to different heights in the gauge.

The number of distinct experiments made with the above described apparatus is eleven; of these, the six first were designed exclusively for the purpose of comparing the vibration in air of full pressure, and in rarefied air. Each experiment consisted of three distinct series of vibrations made in succession, and occupying usually the greater part of two days: the first and third series were in air of full pressure, and the second series in rarefied air; the mean of the results of the first and third series gave the vibration in the ordinary atmosphere, which was compared with the result of the intermediate series in rarefied air. The comparison was thus rendered wholly independent of the daily rate of the clock, and in some measure also of its deviations from an uniform rate in intervals of less than 24 hours. The 7th and 8th experiments had a double object: first, to compare the retardations of a pendulum in common air and in hydrogen gas, both under atmospheric pressure; and secondly, to obtain the amount of retardation in both cases, by comparing the vibration in hydrogen gas under thirty inches pressure, with the vibration in the same gas in a highly rarefied state. There are thus eight experiments on the retardation occasioned by the ordinary atmosphere, and two on the retardation in hydrogen gas under atmospheric pressure; the latter furnishing also the comparative influence of hydrogen gas and atmo-

spheric air. The 9th, 10th, and 11th experiments were made to examine whether the vibration of the pendulum in air within the apparatus was the same as in the free air of the apartment; this was accomplished by vibrating the pendulum alternately in free and in confined air, by removing and again replacing such parts of the apparatus as were necessary for that purpose. It was found, by this means, that the confinement of the medium by the glasses produced no sensible effect on the time of vibration.

From the mean of the eight experiments on the retardation in common air, the author obtains 10·36 vibrations *per diem*, as the reduction to a vacuum of the invariable pendulum, vibrating in air of 45° under a pressure of 30 inches of mercury: and by computing the retardation severally for the circumstances of each experiment, and comparing the computed and observed retardations, he shows, that were the amount of the reduction to a vacuum separately derived from each of the eight experiments, it would in no case differ more than 0·14 of a vibration *per diem*, from the conclusion derived from their mean, or one 74th part of the conclusion itself.

The reduction to a vacuum which would have been previously computed for the vibration in air of 45°, and 30 inches pressure, is 6·26 vibrations *per diem*. The retardation in air of that temperature and density is therefore 4·1 vibrations greater than has been hitherto supposed; and the proportion which the experimental reduction bears to that which is now shown to have been erroneous, is, for the invariable pendulum of the ordinary form used in this country, as 1·655 to 1.

From the experiments in hydrogen gas, under a pressure respectively of 30 inches, and of less than one inch, the retardation of a pendulum vibrating in hydrogen gas of 40° under a barometric pressure of 30 inches, is two vibrations *per diem*. The hydrogen gas employed was obtained by the action of zinc upon dilute sulphuric acid, and was passed into the apparatus through a cylinder containing muriate of lime. A portion withdrawn after the experiments were concluded, was examined by Mr. Faraday, and found to contain no appreciable mixture of air.

The two experiments on the comparative retardation in air and in hydrogen gas give the ratio as 10·55 to 2, and as 10·41 to 2; or, generally, as $5\frac{1}{4}$ to 1. But the ratio of the respective densities of atmospheric air and hydrogen gas is about as 13 to 1. Whence the author takes occasion to remark, that if the resistance of the elastic fluids to bodies falling through them were simply as the respective densities of the fluids, the retardation occasioned by air should be 13 times as great as that occasioned by hydrogen gas,—that the difference of this ratio from that shown by experiment is much too great to be ascribed to error of experiment, particularly as repetition produced results almost identical; that it may rather be regarded as indicating an inherent property in the elastic fluids analogous to that of viscidity in liquids, and of resistance to the motions of bodies passing through them independently of their density; and

that as an example, hydrogen gas, compared with atmospheric air, appears to possess this property in a proportion more than double that which would be given by the respective densities of air and hydrogen gas.

The paper concludes with an investigation of the effect which the near reduction to a vacuum will have on the variations of gravity at different parts of the earth's surface, which have been obtained with invariable pendulums; and particularly of the experiments of the author himself, which embrace a greater range and variety of temperature than those of any other experimentalist. It is shown that in consequence of the peculiar mode in which those experiments were reduced to a mean term of comparison, their re-calculation with the more correct elements now known, would have no other effect than that of adding an equal amount to the vibrations of the experimental pendulum at every station, leaving the acceleration at different stations unaltered.

Consideration of the Objections raised against the geometrical Representation of the Square Roots of Negative Quantities. By the Rev. John Warren, *M.A. of Jesus College, Cambridge. Communicated by* Thomas Young, *M.D. For. Sec. R.S.* Read February 19, 1829. [*Phil. Trans.* 1829, *p.* 241.]

It has always appeared a paradox in mathematics, that by employing what are called imaginary or impossible quantities, and subjecting them to the same algebraic operations as those which are performed on quantities that are real and possible, the results obtained should always prove perfectly correct. The author inferring from this fact, that the operations of algebra are of a more comprehensive nature than its definitions and fundamental principles, was led to inquire what extension might be given to these definitions and principles, so as to render them strictly applicable to quantities of every description, whether real or imaginary. This deficiency, he conceives, may be supplied by having recourse to certain geometrical considerations. By taking into account the directions as well as the lengths of lines drawn in a given plane, from a given point, the addition of such lines may admit of being performed in the same manner as the composition of motions in dynamics; and four such lines may be regarded as proportional, both in length and direction, when they are proportionals in length, and, when also the fourth is inclined to the third at the same angle that the second is to the first. From this principle he deduces, that if a line drawn in any given direction be assumed as a positive quantity, and consequently its opposite a negative quantity, a line drawn at right angles to the positive or negative direction will be represented by the square root of a negative quantity; and a line drawn in an oblique direction will be represented by the sum of two quantities, the one either positive or negative, and the other the square root of a negative quantity. On this subject, the author published a treatise in April 1828; since

which period several objections have been made to this hypothesis. The purpose of the present paper is to answer these objections.

The first of these is, that impossible roots should be considered merely as the indications of some impossible condition, which the proposition that has given rise to them involves; and that they have in fact no real or absolute existence. To this it is replied by the author, that although such a statement may be true in some cases, it is by no means necessarily so in all; and that these quantities resemble in this respect fractional and negative roots, which, whenever they are excluded by the nature of the question, are indeed signs of impossibility, but yet in other cases are admitted to be real and significant quantities. We have therefore no stronger reasons, *à priori*, for denying the real existence of what are called impossible roots, because they are in some cases the signs of impossibility, than we should have for refusing that character to fractional or negative roots on similar grounds.

It has been objected, in the second place, that there is no necessary connexion between algebra and geometry, but only one of analogy; and that it is consequently improper to introduce geometric considerations into questions purely of an algebraic nature. In answer to this, the author contends that a necessary connexion may be shown to exist between impossible roots, and the series expressive of the ratio between the circumference of a circle and its diameter. This he endeavours to prove by examining such values of the expansion of 1^x as are functions of x; whereby he is led to a series, the terms of which involve both the square root of unity, and also the above-mentioned geometric ratio. In other cases he arrives, by methods which are purely algebraic, to expressions containing sines and cosines, together with impossible roots. Hence the author infers that a necessary connexion exists between algebra and geometry; and that his own hypothesis as to the geometric representation of the square roots of negative quantities, is true in the same sense as the hypothesis adopted by algebraists respecting the geometric representation of negative quantities is true.

To a third objection, derived from the alleged inutility of such a geometric representation of the square roots of negative quantities, the author replies, that from their frequent employment by mathematicians, it is reasonable to expect that they will be of much greater use when the true theory of their nature shall be established than when it was unknown.

If the hypothesis of the author is admitted, all questions in dynamics where the motions of bodies are limited to one plane, will be brought within the province of pure algebra.

The author concludes by noticing a work by M. Mourey, entitled "*La vraie Théorie des Quantités Négatives, et des Quantités prétendues Imaginaires*," in which the same general views of the subject are presented as are entertained by the author.

Anatomical Description of the Foot of a Chinese Female. By Bransby Blake Cooper, *Esq., Surgeon to Guy's Hospital. Communicated by* Peter Mark Roget, *M.D. Sec.R.S.* Read March 5, 1829. [*Phil. Trans.* 1829, *p.* 255.]

The foot, of which an account is here given, was obtained from the dead body of a female found floating in the river at Canton, and had all the characters of deformity, consequent upon the prevailing practice of early bandaging, for the purpose of checking its natural growth. To an unpractised eye, it has more the appearance of a congenital malformation, than of being the effect of art, however long continued; and appears at first sight like a club-foot, or an unreduced dislocation. From the heel to the great toe, the length of the foot measures only five inches; the great toe is bent abruptly backwards, and its extremity points directly upwards, while the phalanges of the other toes are doubled in beneath the sole of the foot, leaving scarcely any breadth across the foot, where it is naturally broadest. The heel, instead of projecting backwards, descends in a straight line from the bones of the leg, and imparts a singular appearance to the foot, as if it were kept in a state of permanent extension. From the doubling in of the toes into the sole of the foot, the external edge of the foot is formed in a great measure by the extremities of the metatarsal bones, and a deep cleft or hollow appears in the sole of the foot, across its whole breadth. The author gives a minute anatomical description of all these parts, pointing out the deviations from the natural conformation. He remarks that from the diminutive size of the foot, the height of the instep, the deficiency of breadth, and the density of the cellular texture of the foot, all attempts to walk with so deformed a foot, must be extremely awkward; and that in order to preserve an equilibrium in an erect position, the body must necessarily be bent forwards with a painful effort, and with a very considerable exertion of muscular power.

Some Observations on the Functions of the Nervous System, and the relation which they bear to the other vital Functions. By Alexander Philip Wilson Philip, *M.D. F.R.S. L. & E.* Read April 2, 1829. [*Phil. Trans.* 1829, *p.* 261.]

The intention of the author in the present paper, is, not to bring forwards any new facts, but to take a general review of the inferences deducible from the series of facts detailed by him in previous papers communicated to this Society. He divides the nerves into two classes, essentially differing in their functions. The first comprehends those nerves, which, proceeding directly from the brain and spinal cord to other parts, convey in the one case to those parts the influence of those organs only from which they originate, and thus excite to contraction the muscles of voluntary motion; and in the other case transmit to the sensorium impressions made on the parts to which they are distributed. The second class comprises what may be

termed the Ganglionic nerves, or those which enter ganglions, properly so called; that term being limited to such protuberances only as receive branches of nerves proceeding from the brain and spiral cord. These nerves are distributed more especially to the vital organs, as the thoracic and abdominal viscera, and to the muscles subservient to their functions. The nerves belonging to this class also convey impressions to the sensorium, and occasionally excite the muscles of involuntary motion, which, in common with all muscles, possess an inherent power of contractility dependent solely on their own mechanism, and which in ordinary cases are excited by stimuli peculiar to themselves. But the most important function of the ganglionic nerves, is that of supporting the processes of secretion and assimilation, which require for their performance the combined influence of the whole brain and spinal cord. Viewed as a whole, the system of ganglionic nerves, therefore, constitutes, in the strictest sense, a vital organ. Thus the sensorium, though connected by means of the cerebral and spinal nerves only partially with the organs of sense and voluntary motion, is, by means of the ganglionic nerves, connected generally with all the functions of the animal body. Hence affections of the stomach and other vital organs extend their influence over every part of the frame; while those of a muscle of voluntary motion, or even of an organ of sense, although possessing greater sensibility, are confined to the injured part.

From a due consideration of the phenomena of the nervous system, it would appear that they imply the operation of more than one principle of action. The sensorial power is wholly distinct from the nervous power; the former residing chiefly in the brain, while the latter belongs equally to the spinal cord and brain, and may be exercised independently of the sensorial power. In like manner, the muscular power resides in the muscles, and may be called into action by various irritations independently of the nervous power, though frequently excited by the action of that power. The muscles of voluntary motion are subjected to the sensorial power through the intervention of the nervous system; and those of involuntary motion are also, under certain circumstances, capable of being excited through the nerves by the sensorial power, particularly when under the influence of the passions. The same observation applies also to other actions which properly belong to the nervous power, such as the evolution of caloric from the blood, and the various processes of secretion and of assimilation. That the nervous power is in these instances merely the agent of other powers, and is independent of the peculiar organization of the nerves, is proved by the same effects being produced by galvanism, transmitted through conductors different from the nerves. The successive subordination of these several powers is shown during death, when the sensorial functions are the first to cease, and the animal no longer feels or wills, but yet the nervous power still continues to exist, as is proved by the nerves being capable, when stimulated, of exciting contractions in the muscles, both of voluntary and of involuntary motion,of producing the evolution

of caloric and of renewing the processes of secretion. In like manner the power of contraction, inherent in the muscular fibre, survives the destruction of both the sensorial and nervous powers, having an existence independent of either, although in the entire state of the functions they are subjected to the entire influence of both.

A difficulty here presents itself. If both the nervous and muscular powers be independent of the sensorial power, why, it may be asked, do the more perfect animals survive for so short a time the loss of the sensorial functions? This the author explains by the dependence of respiration on all the three powers,—the sensorial, nervous, and muscular, and its consequent cessation when either of these powers is withdrawn. In support of this view of the subject, he adduces various arguments to show that the muscles of respiration belong to the class of voluntary muscles, and that their action in performing that function is strictly voluntary, and the result of an impression made upon the sensorium by the want of fresh air in the lungs. These actions, though they have become automatic, are originally and essentially voluntary, and remain so even during apoplexy, as long as the breathing continues; but as soon as all sensibility is destroyed they necessarily cease, and death ensues. The phenomena are not explicable upon the hypothesis of a particular sympathy existing in the nerves distributed to the muscles subservient to respiration, and, if the above theory be adopted, require no other supposition for their explanation. As the organs supplied by the ganglionic nerves are subjected to the influence, not of any one but of every part of the brain and spinal cord, no inference respecting the sympathies of any of these nerves can be drawn, either from their particular origin or mode of distribution; and still less room can there be for such inferences in functions, where, as in respiration, the sensorial power is so materially concerned.

On the Respiration of Birds. By William Allen *and* William Hasledine Pepys, *Esqrs. Fellows of the Royal Society.* Read April 30, 1829. [*Phil. Trans.* 1829, *p.* 279.]

The inquiries of the authors on human respiration, and on that of the guinea pig, and of which they communicated the details to the Royal Society in former papers, are here extended to the respiration of birds. Pigeons were the subjects of these experiments, and the same apparatus was employed as the one used for the guinea pig, described in the Philosophical Transactions for 1809.

The object of the first experiment was to ascertain the changes which take place in atmospheric air when breathed by a bird in the most natural manner. For this purpose a pigeon was placed in a glass vessel containing 62 cubic inches of air, and commuuicating with two gasometers, one of which supplied from time to time fresh quantities of air, and the other received portions which become vitiated by respiration. The experiment lasted 69 minutes, and was productive of no injury to the bird excepting a slight appearance of uneasi-

ness whenever the supply of air was not sufficiently rapid. On examining the air at the end of the experiment, no alteration had taken place either in the total volume of air, or in the proportion of azote which it contained; the only perceptible change being the substitution of a certain quantity of carbonic acid for an equal volume of oxygen gas, amounting to about half a cubic inch per minute, and being equivalent to the addition of 96 grains of carbon in 24 hours.

Two experiments were made on the respiration of oxygen gas, obtained from chlorate of potash, and containing in the one case two, and in the other only one per cent. of azote. Under these circumstances it was found that the volume of the gas was unaltered, and that a similar quantity of oxygen gas had been abstracted, but that a much smaller quantity of carbonic acid had been formed than in the last experiment, the remaining portion being made up by azotic gas which had been given out from the lungs of the bird, and the volume of which was just equal to that of the oxygen absorbed. The bird was somewhat distressed during the experiment, but recovered immediately and perfectly on being released from its confinement.

In the fourth experiment, in which a pigeon was made to respire a mixture of oxygen and hydrogen, with a small proportion of azote, (the oxygen being in the same proportion as in common air,) it was found that there was no loss of oxygen; but that a quantity of hydrogen disappeared, and was replaced by an equal volume of azote. The authors observe that birds have a quicker circulation of blood than other animals; and also that they are more sensible to the stimulating effects of oxygen.

On the spontaneous Purification of Thames Water. By John Bostock, *M.D. F.R.S. &c.* Read April 30, 1829. [*Phil. Trans.* 1829, *p.* 287.]

In the report which the author made of the result of his examination of Thames water to the Commissioners appointed by His Majesty to inquire into the supply of water in the metropolis, one of the specimens, taken near the King's Scholars' pond-sewer, was described as in a state of extreme impurity. The water had remained in the laboratory unattended to; but after an interval of some weeks it was observed to have become clear, while nearly the whole of the former sediment had risen to the surface, forming a stratum of half an inch in thickness, and still emitting a very offensive odour. In process of time this scum separated into large masses or flakes, with minute air-bubbles attached to them. At the end of two months longer these masses again subsided, leaving the fluid almost totally free from any visible extraneous matter. On analysis the water was found to contain lime, sulphuric and muriatic acids, and magnesia, in much larger quantities than in the specimens of Thames water previously examined, the proportion of saline matter being increased four-fold. The proportion of the muriates is nearly twelve times greater; that of carbonate of lime between two and three times, and that of sulphate of lime five and a half times greater. The water

in its foul state had given very obvious indications of both sulphur and ammonia; but neither of these substances could be detected after its spontaneous depuration.

The source of these new saline bodies is referrible to the organic substances, chiefly of an animal nature, which are so copiously deposited in the Thames. The depurating process may be denominated a species of fermentation, in which the softer and more soluble animal compounds act as the ferment, and are themselves destroyed, while the salts that were attached to them are left behind. Hence, the more foul the water the more complete the depuration; and it is on this principle that the popular opinion of the peculiar fitness of Thames water for being used at sea may be explained; its extreme impurity inducing a sufficient degree of fermentation to effect the removal of all those substances which might induce any future renewal of that process.

On the Composition of Chloride of Barium. *By* Edward Turner, *M.D. Professor of Chemistry in the University of London. Communicated by Dr.* Dionysius Lardner, *F.R.S.* Read May 14, 1829. [*Phil. Trans.* 1829, *p.* 291.]

The frequent employment of chloride of barium in delicate chemical investigations, renders an exact knowledge of its composition peculiarly desirable; and this has become a more important object of inquiry since it has been made by Dr. Thomson the basis of his calculations of the chemical equivalents of sulphuric acid, and of thirteen metals and their protoxides. He has deduced from his experiments with the chloride of barium the number 36 as the equivalent of chlorine; 70 as that of barium; and 78 as that of baryta; whence the equivalent of the chloride of barium would be 106; and accordingly, on mixing this quantity of the chloride with 88 parts of sulphate of potash, each being previously dissolved in separate portions of distilled water, he finds a complete double decomposition has taken place; the resulting sulphate of baryta, reduced to dryness, weighing 118 parts, and the muriate of potash yielding 76 parts of chloride of potassium. Hence he infers that 40 is the equivalent number for sulphuric acid, and 48 that for potash. Berzelius, however, maintained that this experiment, as well as the deductions from it, are not exact. Dr. Thomson having, in consequence of Berzelius's objections, repeated his experiments, still asserts their accuracy. The author of the present paper investigated the subject with the greatest care, employing materials in a state of perfect purity, and obtained results which coincided with those of Berzelius. He details the precautions he took for ensuring the conditions of perfect purity in the substances with which his experiments were made, and to the neglect of which he traces some of the errors which he imputes to Dr. Thomson's analysis. But there exists also a more radical cause of error in the method employed by that chemist; for Dr. Turner finds that when solutions of muriate of baryta and of sulphate of potash

are mixed together, a small portion of the latter salt adheres tenaciously to the sulphate of baryta, which is precipitated, and escapes decomposition. By employing different processes the author avoids this source of fallacy; first, from the chloride of barium, previously dissolved in water, he throws down sulphate of baryta by adding sulphuric acid; and, secondly, he effects a precipitation from a similar solution of the chloride, by nitrate of silver, and infers the quantity of chloride from that of the fused horn-silver obtained, having previously determined, by a separate series of experiments, the exact composition of horn-silver. The conclusion he draws from his researches is, that 100 parts of chloride of barium correspond to 137·63 parts of the chloride of silver, which latter substance contains 34·016 parts of chlorine, and therefore leaves for the proportion of barium 65·984 parts. The real equivalent of barium, however, will depend upon that of chlorine, which is itself not yet satisfactorily determined.

On a new Series of periodical Colours produced by the grooved Surfaces of Metallic and Transparent Bodies. By David Brewster, *LL.D. F.R.S. L. & E.* Read May 21, 1829. [*Phil. Trans.* 1829, *p.* 301.]

The author, having received from Mr. Barton in the year 1822 some fine specimens of his Iris ornaments, undertook a series of experiments on the action of grooved surfaces upon light, of which he communicated an account to the Royal Society of Edinburgh in the following year. The investigation having since been taken up by Mr. Fraunhofer, the author had desisted from pursuing it until lately, when he learned that the phenomena which had principally occupied his attention had escaped the notice of this philosopher. The image of a candle seen by reflexion, from a flat and polished metallic surface, covered with equal and equidistant grooves, the plane of reflexion being parallel to the grooves, is accompanied with a row of prismatic images, arranged in a line perpendicular to the grooves. The colourless image of the candle is formed by the original portions of the metallic surface, which have been left between the grooves, while the prismatic images are formed by the sides of the grooves themselves. This is rendered evident to the eye by varying the proportion between these two parts of the surface. The general phenomena of the prismatic images, such as their distance from the ordinary image, and the dispersion of their colours, depend entirely on the number of grooves and intervals which occupy a given breadth; and the laws of these phenomena have been accurately determined by Mr. Fraunhofer. Dr. Brewster, by examining the appearances with more attention, observed in some specimens a remarkable defalcation of particular colours, varying with the angle of incidence, and sometimes affecting one of the images and not the others; in some cases even the image reflected from the original surface of the steel was slightly coloured, its tint having a

relation to the defalcation of colour in the prismatic images. In order to observe these phenomena through a great range of incidence, he employed a long narrow rectangular aperture, which gave a convergent beam of 30° or 40°. Under these circumstances, the ordinary image of the aperture, as formed by the original surfaces, was crossed, in a direction at right angles to its length, with broad coloured fringes, varying in their tints according to the angle of incidence. In a specimen having 1000 grooves in an inch, no less than four complete orders of colours are developed, corresponding to those of the reflected rings of thin plates. By turning the steel plate round in azimuth, the same colours are seen at the same angles of incidence, and they undergo no change by varying the distance of the luminous aperture, or of the eye of the observer.

The analysis of these curious and apparently complicated phenomena is much simplified by the employment of homogeneous light. The author pursues this analysis with red and with violet light respectively, and explains the obliteration of the colours by the aid of diagrams, giving also various tables of the angles of incidence at which the several deficiencies occur in the reflected colours. These angles are rendered different by covering the steel plate with water and oil of cassia in succession. Phenomena analogous to those above described take place on the grooved surfaces of gold, silver, calcareous spar, and other substances. Similar grooves impressed upon tin, realgar, and also upon isinglass, exhibited phenomena diversified according to the respective refractive powers of these substances. The almost perfect transparency of isinglass enabled the author to examine the transmitted tints, which in the ordinary image he found were extremely brilliant, but had no relation whatsoever, either in number or in quality, to the reflected tints. The transmitted tints of the ordinary prismatic images always increase in brightness as the angle of incidence diminishes; while the reflected tints become fainter.

The new class of periodical colours described in this paper cannot, in the opinion of the author, be referred to the diffraction and interference of the rays reflected from two or more of the portions of the original surface of the metal, considered as narrow slits or apertures; because they would in that case be affected by the distance both of the luminous object and of the eye, and the colours would form bands parallel to the direction of the grooves. But if we suppose that the parts of the original surface are smaller than the distance to which the reflecting force extends, the removal of the metal from the adjacent grooves must diminish the reflecting force of these parts of the surface; and he infers, from direct experiment, that this is the case.

On the hypothesis of emission, this abstraction of reflecting matter may be regarded as equivalent to a diminution of the density of the surface; while on the undulatory hypothesis, the effect may be ascribed to the condition of the ether arising from a variation in its intensity or elasticity towards the surface of a number of salient points.

On the Nerves of the Face; being a second Paper on that Subject. *By* Charles Bell, *Esq. F.R.S.* Read May 28, 1829. [*Phil. Trans.* 1829, *p.* 317.]

After recapitulating the contents of his former paper, the author cites cases which have occurred since its publication, in support of his doctrine, first, that the sensibility of the head and face depends on the fifth pair of nerves; secondly, that the muscular branches of that pair are subservient to mastication; and, thirdly, that the portio dura of the seventh pair controls those motions of the parts of the face, whether voluntary or involuntary, which are connected with respiration. Instances are given of lesions of the portio dura, from accident or from disease, followed by paralysis of the muscles on the same side of the face, while the sensibility remained. On the other hand, cases are related of injury to the fifth pair, being attended with loss of sensibility in all the parts receiving branches from the injured nerve, while the power of motion continued unimpaired. In one case of this description, where one half of the under lip had become insensible, on a tumbler being applied to the mouth, the patient imagined it was a broken glass that he touched. A similar delusion was experienced by another patient, in whom the half of the upper lip had been deprived of sensation by an injury to the sub-orbital branch on the same side. From these facts the author deduces the absurdity of the practice of cutting the portio dura for the relief of tic douleureux.

He next enters into an anatomical description of the course of that division of the fifth pair of nerves, which is unconnected with the Gasserian ganglion, and passes under it, and which he considers as the motor or manducatory portion of the fifth, being distributed to the temporal, masseter, pterygoid, and buccinator muscles; some branches of it supplying the muscles of the lips, and also the mylohyoideus and anterior belly of the digastricus, the action of which is to depress the jaw.

In proof that this nerve is destined to manducation, the root of the fifth pair being exposed in an ass, and irritated, the jaws closed with a snap; and on its being divided, the jaw fell relaxed and powerless.

The author next endeavours to show the necessity of an accordance between the motions of the lower jaw and those of the cheeks during mastication; and the probability that this connexion of motions is brought about by means of the connexions which exist among their respective nerves, and between which a sympathy may in consequence be established. In one case violent spasms took place in the masseter and temporal muscles, while the motions of the features were free and unconstrained; and in another, the muscles of the jaw on one side were paralysed, with loss of sensibility on that side of the face. On the other hand, when the portio dura was paralysed, all the muscles of the face wasted, except those supplied by the fifth pair.

The author concludes by a warm tribute of acknowledgement to

his brother-in-law, the late Mr. Shaw, for the valuable assistance he afforded him in the whole course of his investigations on the nerves.

On the Reduction to a Vacuum of Captain Kater's *convertible Pendulum. By Captain* Edward Sabine, *of the Royal Artillery, Secretary to the Royal Society.* Read June 18, 1829. [*Phil. Trans.* 1829, *p.* 331.]

Recent investigations having shown that the method employed by Captain Kater for the reduction of his experiments on the length of the pendulum vibrating seconds in air, to that of the same pendulum *in vacuo*, was founded on erroneous principles, the author undertook to ascertain, by direct experiment, the actual difference of the number of vibrations of the pendulum employed by Captain Kater, in air of ordinary density, and in highly rarefied air. The alteration of density in the medium in which the pendulum is swung, would, in the first place, if its form were not symmetrical, affect its convertibility; that is, the same adjustment of the axes which gave an equality of oscillations in reversed positions, when vibrating in air, would not afford the same equality in a more rarefied medium. It follows also, from the corrected investigation, that the amount of the retardation occasioned by the air is considerably greater than what had been originally computed from the simple consideration of buoyancy.

These inferences have been fully confirmed by the experiments of Captain Sabine. The increase in the number of vibrations *per diem* with the convertible pendulum as it was used by Capt. Kater, that is, vibrating with the great weight below, *in vacuo*, above those in air of the temperature of 49°, under a pressure of 30 inches of mercury at 32°, was 15·71: when inverted, the other conditions remaining the same, the increase was 16·13 vibrations *per diem*.

Captain Kater had observed that considerable changes in the hygrometric state of the atmosphere destroyed the convertibility of his pendulum, from their affecting the weight of the pieces of wood at both of its ends. In order to remove this source of error, and also to ascertain its amount, the author first reduced the wooden tail-pieces from 17 inches, their original length, to 6·4 inches. The increase of the number of vibrations was then, with the great weight above, 14·91, and with the great weight below, 12·41 *per diem*. When the wooden tail-pieces were wholly removed, and slips of brass substituted for them, the increase was further reduced, in like circumstances, to 12·83 in the former case, and 11·58 in the latter.

On the geometrical Representation of the Powers of Quantities, whose Indices involve the Square Roots of negative Quantities. By the Rev. John Warren, *M.A. late Fellow and Tutor of Jesus College, Cambridge. Communicated by the President.* Read June 4, 1829. [*Phil. Trans.* 1829, *p.* 339.]

The author, in a former paper, read to the Society in February last, had discussed various objections which had been raised against his mode of geometric representation of the square roots of negative quantities. At that time he had only discovered geometrical representations for quantities of the form $a + b\sqrt{-1}$, of geometrically adding and multiplying such quantities, and also of raising them to powers either whole or fractional, positive or negative; but he was at that time unable to represent geometrically quantities raised to powers, whose indices involve the square roots of negative quantities (such as $\overline{a + b\sqrt{-1}}^{\,m+n\sqrt{-1}}$). His attention has since been drawn to this latter class of quantities by a passage in M. Mourey's work on this subject, which implied that that gentleman was in possession of methods of representing them geometrically, but that he was at present precluded by circumstances from publishing his discoveries. The author was therefore induced to pursue his own investigations, and arrived at the general result stated by M. Mourey, that all algebraic quantities whatsoever are capable of geometrical representation by lines all situated in the same plane. The object of the present paper is to extend the geometrical representations stated in his former treatise, to the powers of quantities, whose indices involve the square roots of negative quantities. With this view he investigates various equivalent formulæ suited to the particular cases, and employs a peculiar notation adapted to this express purpose; but the nature of these investigations is such as renders them incapable of abridgement.

An experimental Examination of the Electric and Chemical Theories of Galvanism. By William Ritchie, *A.M. F.R.S. Rector of the Royal Academy at Tain.* Read May 7, 1829. [*Phil. Trans.* 1829, *p.* 361.]

After observing that the theory of galvanism originally proposed by Volta, and generally termed the Electric theory, is still the universally received doctrine among continental philosophers, the author adduces several experiments proving the fallacy of the principles on which that theory is founded. He points out the inconclusiveness of the reasoning by which it has been inferred that dissimilar metals, by being simply placed in contact with one another, are instantly thrown into opposite electric states; for in all the experiments which have been made with a view of establishing this fundamental principle of the electric theory, the metals have been exposed to the oxidizing action of the air, which is a constant source of electric disturbance, and the operation of which has been strangely overlooked. The

author found, by forming galvanic circles with two different metals and an interposed acid, that when he used different kinds of acid, or varied the degree of their dilution, the electro-magnetic effects, as measured by a delicate galvanometer, bear no sort of relation to the conducting power of the fluid, as is assumed in the Voltaic hypothesis. He deduces the same conclusion from experiments made with an apparatus by which the fluid is confined in a rectangular box, divided by a membranous diaphragm into two compartments, so as to allow of the addition of an acid to the fluid contained in one of the compartments, and thereby limiting its action to one of the metallic surfaces. By means of another contrivance, the author ascertained that of two different metals, the one which, when acted upon by an acid, combines with the greatest quantity of oxygen, as measured by the volume of hydrogen disengaged, is always positive with respect to the other metal. Even two pieces of the same metal, differing in hardness, will be acted upon by the same acid in different degrees, and may thus be brought into different states of electricity. In general it is the harder of the two pieces of metal which becomes positive; but with steel the reverse obtains. It would appear, however, that with the same pairs of metallic discs, the direction of the electric current is determined by the nature of the acid employed: thus nitrous acid, acting upon zinc, copper, or iron, gives rise to a current in a direction opposite to the current which is produced by the sulphuric, nitric, or muriatic acids. Variations in the temperature of the metals will also occasion diversities in the results, not hitherto satisfactorily explained on any theory. From one experiment the author is led to infer that an acid is capable of combining with a pure metal, without the latter being previously reduced to the state of an oxide.

The Bakerian Lecture. On the Manufacture of Glass for Optical Purposes. By Michael Faraday, *Esq. F.R.S. &c.* Read November 19, December 3 and 10, 1829. [*Phil. Trans.* 1830, *p.* 1.]

As an introduction to his paper, the author gives a short account of the circumstances which have led to the present inquiry. He states the difficulties that exist in procuring glass sufficiently homogeneous to answer the purposes of the optician, and adverts to the efforts made by Guinand and by Fraunhofer to overcome them. As the art was still imperfectly known in this country, the President of the Royal Society in the year 1824 suggested the appointment of a committee, whose labours were facilitated by the Government removing the restrictions imposed by the excise laws to experiments on glass, and also undertaking to bear all the expenses of the inquiry, as long as it held out a reasonable expectation of ultimate success. An experimental glass-house was at first erected on the premises of Messrs. Pellatt and Green, at the Falcon Glass-works; but Mr. Faraday being unable to conduct them at that distance from his own residence, the President and Council of the Royal Society obtained leave

of the President and Managers of the Royal Institution to erect another experimental furnace for continuing the investigation on their premises.

The author being intrusted with the immediate superintendence of the experimental part of the manufacture of the glass, conceives it to be his especial duty, at the present stage of the inquiry, to give an account of what has been done in his department; for although the investigation is still far from being completed, yet he trusts that a decided step has now been made in the manufacture of glass for optical purposes; and that it is due to the Society, as well as to the Government, to render an account of the results hitherto obtained.

The author begins this account by a statement of the usual defects incident to glass, which destroy the regularity of its action on light. These are, on the one hand, streaks, striæ, veins, and tails; and, on the other hand, minute bubbles; the former arising from the want of homogeneity, the latter from the intermixture of air. Of these, the first class of defects constitute the most serious evil, as they interfere with the rectilineal course of the rays of light while traversing the glass, while the latter are injurious merely from the interception of the rays, and their dispersion in all directions. The greater the difference in specific gravity of the ingredients of the glass, the greater is the tendency to form striæ when they are fused together; hence flint glass, which contains a large proportion of lead, is more liable to this defect than either crown- or plate-glass. After numerous trials of materials different from those which enter into the composition of the ordinary kinds of glass, borate of lead and silica were fixed upon as the most eligible, and as near an approximation as possible to a definite chemical union of their elements was aimed at, by taking single proportionals of each, and endeavouring to procure them, previous to combination, in the greatest possible state of purity. The oxide of lead was obtained from the nitrate of the metal previously crystallized. The boracic acid was also selected from the purest crystals afforded by the manufacturer, and carefully tested to ascertain its freedom from foreign matters. The silica employed was that of flint glass-maker's sand, obtained from the coast of Norfolk, and well washed and calcined, and freed from iron by nitric acid. It was then combined with protoxide of lead. These materials were then mixed in the proportions of 154·14 parts of nitrate of lead, 24 of silicate of lead, and 42 of crystallized boracic acid, and melted together in a separate furnace adapted expressly for this preliminary operation, and of which a minute description is given. A tray was then prepared of a thin lamina of platina, all the apertures of which were carefully closed by soldering, for containing the pulverized glass, which was to be subjected to the final melting in a furnace of peculiar construction, which the author terms the finishing furnace. After numerous trials of substances for constructing the chamber in which the fusion of the glass contained in the tray was to be conducted, recourse was had to the materials from which the Cornish crucibles are manufactured, and which were obtained through the kindness of the

President, and were expressly manufactured for the purpose by Mr. Mitchell, of Caleneck in Cornwall. In order to prevent the reduction of any portion of the lead entering into the composition of the glass, a current of fresh air was introduced by a tube, and made to pass along the surface of the fused glass. A very minute and circumstantial account is given of all the manipulations necessary for conducting these processes in all their stages; in some of which, however, the best methods of proceeding still remain to be ascertained, variations having been made up to the very last experiment, and it is only by still more extensive experience that the author expects the proper arrangements will ultimately be settled. Directions are given as to the occasional inspection of the glass during the process, the mode of stirring by a rake of platina, and the plan devised by the author of accelerating the disengagement and escape of bubbles, by throwing into the melted materials a quantity of pulverized platina, mixed with fragments of the same kind of glass. The glass which has been obtained by the mixture of materials above mentioned, constituting silicated borate of lead, has a specific gravity of 5·44, and high refractive and dispersive powers, and perhaps also very considerable reflecting power.—It is softer than ordinary glass, but less liable to be tarnished by sulphureous vapours, as they commonly exist in the atmosphere; and also less acted upon by moisture than glass, in which potash enters as an ingredient; it is also a much more perfect electric than ordinary glass. An Appendix is subjoined, containing descriptions of the rough glass furnace, and the finishing furnace; and also directions for preparing the spongy platina employed by the author in the latter stage of the process, in order to promote the disengagement of bubbles.

Account of Levellings carried across the Isthmus of Panamá, to ascertain the relative Height of the Pacific Ocean at Panamá, and of the Atlantic at the mouth of the River Chagres; accompanied by Geographical and Topographical Notices of the Isthmus. By John Augustus Lloyd, *Esq. Communicated by Captain* Sabine, *Sec. R.S.* Read November 26, 1829. [*Phil. Trans.* 1830, *p.* 59.]

The author having received from General Bolivar a special commission to survey the Isthmus of Panamá, with the view of ascertaining the most eligible line of communication between the two seas, arrived at Panamá in March 1828. Here he was joined by Captain Falmarc, a Swedish officer of Engineers, in the Colombian service. Anxious to lose no time in the prosecution of their objects, they proceeded on the 5th of May to commence their operations, resolving not to be deterred by the difficulties likely to arise from the rainy season, which had just set in, from personal privations, and even from the dangers to which they might expose their health. Their line of survey commenced at Panamá, and was continued along the old road to Porto Velo till it came to the bed of the Chagres, a river which falls into the Gulf of Mexico. The greatest height

passed over in this line was 633·32 feet above the level of high water at Panamá. Their constitutions were now beginning to suffer from the continued exposure to rain, and they therefore determined, after building a secure station on the banks of the Chagres, to defer all future operations till the ensuing year, when the dry season should be established. On the 7th of February, 1829, they resumed their labours, carrying on their levels from a point of the river below their former station, and 152·55 feet above high-water mark at Panamá, along the course of the river to a place distant about 12 miles from its mouth, called La Bruja, where the water in dry seasons is very brackish, and from which there is no perceptible current to the sea.

The result of this survey fixes the mean height of the Pacific, at Panamá, at 3·52 feet above the Atlantic at Chagres. Between the extremes of elevation and depression of the greatest tides in the Pacific at Panamá, there is a difference of 27·44 feet; but the mean difference at the usual spring tides is 21·22. At Chagres, this difference is only 1·16 foot, and is the same at all seasons of the year. Hence it follows, that at high water, the time of which is nearly the same on both sides of the Isthmus, the Pacific is raised, at mean tides, 10·61 feet, and the Atlantic 0·58 foot, above their respective mean levels, giving to the former an elevation above the latter of 13·55 feet. At low water, both seas being below their respective mean levels, by the same quantities as before stated, the Pacific will be lower than the Atlantic by 6·51 feet; so that thus, in the course of every interval from one high tide to the succeeding one, the level of the Pacific is at first higher, then equal, and afterwards lower than the Atlantic; and then again passing back by the same steps it regains its former elevation as the tide returns.

The great chain of mountains which extends from the Andes in South America to the Mexican and Rocky Mountains in North America, is not, as is generally supposed, absolutely continuous through the Isthmus connecting these two continents; for the northern Cordillera on the eastern side of the province of Veragua, breaks into detached mountains of considerable height, having steep and rugged sides. To these succeed numerous conical mountains, rising from plains and savannahs, and seldom exceeding from 300 to 500 feet in height. Between Chagres on the Atlantic side, and Chorrera on the Pacific, the conical mountains are less numerous, and are separated by extensive plains, with only a few occasional insulated hills of inferior extent and elevation. Thus it happens that at the narrowest part of the Isthmus, a break occurs in the mountain-chain, which, in almost every other part, is uninterrupted from its northern to its southern extremities; a circumstance which marks this spot as peculiarly adapted for the establishment of a communication across. The author has laid down on his map two lines for a rail-road, both commencing at a point near the junction of the river Trinidad with the Chagres, and crossing the intervening plain, the one to Chorrera, the other to Panamá. The latter line, although the longer of the two, would have the advantage of terminating in a considerable city.

The banks of the river Trinidad are represented by the author as being well suited for wharfs, especially in the neighbourhood of the spot he recommends as the commencement of a rail-road; but as the mouth of the Chagres is impeded by a bar, he suggests the expediency of forming a communication with the adjacent Bay of Limon, which, in its present state, affords excellent anchorage, and which, by making certain improvements in it, pointed out in the paper, might, at a small expense, be rendered one of the most commodious and safe harbours in the world.

On the Law of the partial Polarization of Light by Reflexion. By David Brewster, *LL.D. F.R.S. L. & E.* Read February, 4, 1830. [*Phil. Trans.* 1830, *p.* 69.]

When a beam of ordinary light is incident upon the surface of a non-metallic body, at an angle having a certain relation with its refractive density, the portion which is reflected is found to be completely polarized in the plane of reflexion; but when the angle of incidence is either greater or less than this, which is the polarizing angle, the polarization is incomplete, as is proved by transmitting the reflected beam through a doubly-refracting crystal, which decomposes it into the ordinary and extraordinary rays. What a single reflexion is unable to effect, may, however, be accomplished by a sufficient number of successive reflexions at angles different from the polarizing angle. The author had pointed out this fact in his communications to the Royal Society in the year 1815; and the further investigation of the law on which this phenomenon depends, has led him to the discovery of the real change effected in light by its reflexion, and opened new views of that condition which constitutes its polarization. The prevailing notion with regard to the condition of the light which has been imperfectly polarized by reflexion, has been, that it consisted of two portions; the one wholly polarized in the plane of reflexion, and the other not changed, but still retaining the character of natural light. This doctrine was supported by Young, Biot, Arago, and Fresnel, and more recently by Herschel. Dr. Brewster contends, in opposition to these authorities, that every portion of the reflected beam has suffered a physical change by the action of the reflecting forces, some being completely polarized, others only partially so; complete polarization consisting in the effecting of such a change in the position of the plane of polarization, as that they shall be parallel to the plane of reflexion; partial polarization, on the other hand, consisting in these planes being only brought nearer to this position of parallelism. In order to simplify the investigation, the author begins by considering the case of a beam of light composed of two polarized pencils, of which the respective planes of polarization are at right angles to each other, which two pencils may be conceived to be superposed upon each other. He then shows that the phenomena exhibited by this compound pencil are exactly the same as those exhibited by common or unpolarized light. He next proceeds

to the analysis of a pencil thus composed, after it has suffered reflexion at different angles of incidence; and for this purpose makes the plane of reflexion bisect the right angle formed by the plane of polarization of the two pencils composing the beam. As the angle of incidence diminishes from 90°, these latter planes are gradually turned more and more towards the plane of reflexion; so that the angles they form together become more and more acute, until the incidence becomes that of the angle of complete polarization, in the particular medium from which the reflexion takes place; in that case, the planes become parallel, and the whole of the beam is then completely polarized. As the angle of incidence still further diminishes, these planes become again inclined to one another, but in contrary directions. Thus the total polarization of the reflected pencil at the polarizing angle is effected by the turning round of the planes of polarization of one half of the light from right to left, and of the other half from left to right, each through an angle of 45°. But when the pencil is only partially polarized, each plane of polarization has been turned round in opposite directions, from an inclination of 45°, to one either less or greater than this. The light has, in this case, suffered a physical change of a remarkable kind; for it now constitutes neither natural nor polarized light, but something intermediate between both. It is not the former, because its planes of polarization are not rectangular; nor the latter, because they are not parallel. The examination of a pencil of this description by a doubly-refracting medium, which was the test employed by those who conceived the polarization to be complete in one portion while the remaining portion was wholly unpolarized, does not afford the means of deciding this question; for the results, as the author shows, would be the same in either case. By applying the law of repartition of light, when doubly refracted, between the ordinary and extraordinary rays discovered by Malus, namely, that it follows the duplicate ratio of the sine and cosine of the angle of inclination to the principal section of the crystal, we obtain the same expression for the intensities of both rays, whichever of the hypotheses we adopt. But the proof which a single reflexion is unable to afford is supplied by examining the results of a succession of reflexions. We are thus furnished with means of comparing with great precision the deductions from theory with the results of experiment, and of establishing the correctness or fallacy of both hypotheses. This investigation is pursued by the author in the body of the paper, in considerable detail. He finds the formula given by Fresnel, expressing the law of change in the plane of polarization by reflexion, to be perfectly conformable with observation, the results of which it also expresses with great accuracy when applied to rays which have undergone partial polarization. He controverts the accuracy of a proposition advanced by Arago, that equal proportions of light are polarized at equal angular distances from the angle of complete polarization. As the index of refraction differs for the different colours of the spectrum, the polarizing angle will be different in each. In bodies of high

dispersive powers, accordingly, the unpolarized light which remains in the extraordinary image must be coloured at all incidences, the colours being most distinct near the maximum polarizing angle. This necessary result of the formula, the author finds to be experimentally true in oil of cassia, and various highly dispersive bodies; hence there can be no angle of complete polarization for white light.

The same law which determines the polarization of light by reflexion is applicable also to that by refraction; in both cases, the analysing doubly-refracting crystal is insufficient to distinguish light completely polarized from light in a state of approach to polarization. The difference, however, between these two kinds of light, is marked by most distinctive characters, and shows itself in some of the most complex phenomena of interference. Hence the author is led to consider common light as composed of rays in every state of positive and negative polarization, or of particles having planes, which are acted upon by the attractive and repulsive forces residing in solid bodies; such planes having every possible variety of inclination to a plane passing through the direction of their motion. The formulæ given in the paper represent the laws according to which the repulsive and attractive forces change the position of the planes of polarization; so that the author regards all the various phenomena of the polarization of light by reflexion and refraction, as now brought under the dominion of laws as well determined as those which regulate the motions of the planets.

A Report on the Stomach of the Zariffa. By Sir Everard Home, *Bart. V.P.R.S.* Read December 24, 1829. [*Phil. Trans.* 1830, *p.* 85.]

In common with other ruminant quadrupeds, the Zariffa has a stomach consisting of four cavities. The efflorescence which lines the paunch is similar to that of the bullock, but is more prominent; the second cavity is destitute of the cellular structure met with in other ruminants, but the third and fourth cavities exhibit no peculiarities; the cud formed from the leaves and twigs of the acacia, which are the natural food of the Zariffa, is so succulent, as not to require being again moistened in passing through the second stomach, as is the case with grass; this cavity is therefore not furnished with the cells which are provided for this purpose in herbivorous quadrupeds.

Three drawings of the structure of the parts described accompany the paper.

On the Production of regular double Refraction in the Molecules of Bodies by simple Pressure; with Observations on the Origin of the doubly refracting Structure. By David Brewster, *LL.D. F.R.S. L. & E.* Read February 11, 1830. [*Phil. Trans.* 1830, *p.* 87.]

The author has already shown, in former papers which have appeared in the Philosophical Transactions, that the phenomena of

double refraction may be produced artificially by effecting certain changes in the mechanical condition of hard and of soft bodies. In all these cases, he observes, the phenomena are entirely different from those of regular crystals; and in none of them is the doubly-refracting force a function of the angle which the incident ray forms with one or more axes given in position. In the year 1815, he noticed the depolarizing properties of a thin film of a mixture of resin and white wax, compressed between two pieces of glass. Accidentally meeting with the specimen which had originally been the subject of this observation, he found that after fifteen years it still retained this property of depolarization, and was induced to pursue the inquiry to which it led. He varied the proportions of the ingredients, and observed in the different cases the modifications produced in the phenomena by employing various degrees of pressure. He found that, in every point, there existed an axis of double refraction, perpendicular to the plane of the film, and that the doubly-refracting force varied with the inclination of the incident ray to this axis, just as happens with all regular uniaxal crystals. He infers from his observations, that the property of uniaxal double refraction is communicated to the molecules simply by the agency of pressure; for in all cases where pressure has not operated, the aggregate does not exhibit this property. These effects are precisely the same as those which would be produced by subjecting elastic spheres to a regular compressing force, the axis of pressure becoming an axis of positive double refraction; while extension, on the contrary, produces a negative axis.

From the consideration of the preceding facts, the author is led to a very simple explanation of the origin and general phenomena of double refraction in regular crystals. He considers this property as not being inherent in the molecules themselves, but as resulting from their compression, either by an extraneous force, or by their power of inherent attraction of aggregation. The phenomena of crystallization, and of cleavage, prove that the molecules of crystals have several axes of attraction, or lines, along which they are most powerfully attracted, and in the directions of which they cohere with different degrees of force. Guided by the indications of hemitrope forms, and supposing the molecules to be spherical or spheroidal, it is inferred that these axes are three in number, and at right angles to each other, and that they are related in position to the geometrical axis of the primitive form. In like manner, the phenomena of double refraction are related to the same axis of the primitive form, and may be all rigorously calculated by a reference to three rectangular axes. The author pursues the consequences of these principles in their application to various kinds of crystals. It follows from this theory, that the forms of the ultimate molecules of crystals, existing separately, determine, within certain limits, the primitive form to which they belong; while the doubly-refracting structure, and the precise form of the crystal, are simultaneously produced by the action of the forces of aggregation. These views receive a re-

markable confirmation in the doubly-refracting structure which the author discovered in chabasie; and they also enable us to understand the nature of that influence which heat produces on doubly-refracting crystals, as discovered by Prof. Mitscherlich. The optical phenomena exhibited by fluids under the influence of heat and pressure, and by crystals exposed to compressing or dilating forces, are also in perfect conformity with the above views, and would in themselves have been sufficient to establish the principle that the forces of double refraction are not resident in the molecules themselves, but are the immediate result of those mechanical forces by which these molecules constitute solid bodies.

Experiments on the Influence of the Aurora Borealis on the Magnetic Needle. By the Rev. James Farquharson, *F.R.S. Minister of Alford, Aberdeenshire. In Letters addressed to Captain* Edward Sabine, *Sec. R.S.* Read January 28, March 4, and April 1, 1830. [*Phil. Trans.* 1830, *p.* 97.]

In the first letter, dated from Alford, Dec. 15, 1829, the author gives a description of the instrument which was furnished to him by the Royal Society for measuring the variation of the magnetic needle, and also the magnetic intensity; and of his mode of using it. The needle was so delicately suspended as to render changes in the declination as small as 10″ very sensible. In his experiments on the magnetic intensity, the intervals of time occupied in the needle's performing 50 oscillations, commencing with an arc of 12°, were noted by a stop-watch, in which the stop, being applied on the balance, is instantaneous in its operation. The watch is again released from the stop at the commencement of a new observation; thus compensating, on the principle of the repeating circle, for any inaccuracy in the reading off, or any inequality in the divisions of the dial-plate.

The observations made on an Aurora borealis which appeared on the night of the 14th of December, are particularly detailed. On that occasion, the disturbance of the magnetic declination was so great, and so frequently changing from east to west, and the reverse, as to leave no doubt in the mind of the author of the reality of this influence. The needle, however, was affected at those times only when the fringes of the aurora were in such a position as to include the needle in their planes. It appeared to him, also, that the side towards which the needle declined, was the quarter where the aurora gave out the most vivid light.

His experiments on the oscillations of the needle have not yet enabled him to determine satisfactorily, whether any change of magnetic intensity accompanied these changes of direction.

In a second letter, dated December 26, he gives the results of later observations. From a comparison of his own with the observations of the Rev. James Paull, minister of Tullynessle, he infers that the height of the particular aurora which was seen by them on the 20th, did

not at its upper extremities exceed 4000 feet above the ground; and is led to the general conclusion, that the aurora borealis is situated in the region immediately above the clouds, and therefore varies much in height according to the different states of the atmosphere. He believes it to be an effect of the developement of electricity from the condensation of vapour. The position of the fringes, which are constantly at right angles to the magnetic meridian, their progressive movements from the north magnetic pole, and their influence on the needle whenever they come into the plane of the dip, are all of them circumstances which establish the relation of this phenomenon to magnetism; while they at the same time illustrate the intimate connexion subsisting between magnetism and electricity.

Remarks on several Icebergs which have been met with in unusually low Latitudes in the Southern Hemisphere. By Captain James Horsburgh, *Hydrographer to the East India Company, F.R.S.* Read February 4, 1830. [*Phil. Trans.* 1830, *p.* 117.]

The journals of the ships belonging to the East India Company, the author observes, during the whole of the last century, contain no accounts of icebergs having been seen in the course of their navigation in the southern hemisphere, although several of these ships proceeded into the parallels of latitude 40°, 41°, and 42° south; but during the last two years, it appears that icebergs have occasionally been met with by several ships in their passage, very near the Cape of Good Hope, between the latitudes of 36° and 39°. The particulars relating to these observations are detailed in the paper. The most remarkable occurred in the voyage of the brig Eliza, from Antwerp, bound to Batavia, which on the 28th of April, 1828, fell in with five icebergs in latitude 37° 31′ south, longitude 18° 17′ east of Greenwich. They had the appearance of church steeples, of a height from 250 to 300 feet; and the sea broke so violently against these enormous masses, that it was at first suspected they might be fixed upon some unknown shoal, until, on sounding, no bottom could be discovered.

It is remarkable that in general, icebergs appear to be met with in low latitudes, nearly at the same period of the year, namely, in April or May, in both the northern and southern hemispheres, although the seasons are reversed in these two divisions of the globe. In order to account for the origin and accretion of the southern icebergs, the author thinks it probable that there exists a large tract of land near the antarctic circle, somewhere between the meridian of London and the twentieth degree of east longitude; whence these icebergs have been carried in a north and north-north-easterly direction, by the united forces of current, winds, and waves, prevailing from south-south-west and south-west. Bouvet's and Thompson's Islands are not of sufficient magnitude, and Sandwich Land and Kerguelen's Island are too remote to be the source of the icebergs lately observed in the vicinity of the Cape. From their unprecedented descent during the

last two years, it is most probable that the disruption of these masses of ice from the place of their formation was the effect of some powerful cause of rare occurrence, such as an earthquake or volcano, which has burst forth and convulsed the inaccessible regions of the south; leaving no other testimonials of the event, than some few fragments of ice, scattered at a distance in the Indian Ocean.

On the progressive Improvements made in the Efficiency of Steam Engines in Cornwall; with Investigations of the Methods best adapted for imparting great angular Velocities. By Davies Gilbert, *Esq. P.R.S.* Read March 4, 1830. [*Phil. Trans.* 1830, *p.* 121.]

The practical adaptation of the steam-engine to mechanical purposes is considered by the author as due to Mr. Newcomen, whose engines were introduced into Cornwall very early in the last century, and soon superseded the rude machinery which had till then been employed for raising water from the mines by the labour of men and of horses. The terms proposed by Mr. Watt, in virtue of his patent in 1769, which secured to him, until the year 1800, the receipts of one third of all the savings in fuel resulting from the adoption of his improvements in the construction of the engine, rendered it necessary to institute an accurate comparison between the efficiency of his with former engines. A copy of the report drawn up on this occasion, in October 1778, is given in the paper; but as the dynamic unit of one pound avoirdupois, raised through a height of one foot, had not yet been established as the measure of efficiency, the author, proceeding upon the data furnished by that report, calculates that the duty performed by Watt's engine, with the consumption of one bushel of coal, on that occasion was 7,037,800. In the year 1793, an account was taken of the work performed by seventeen engines on Mr. Watt's construction, then working in Cornwall, their average duty was 19,569,000; which exceeds the performance of the former atmospheric engines, in the standard experiments, in the proportion of 2·78 to 1. Some years afterwards, disputes having arisen as to the real performance of Mr. Watt's engines, the matter was referred to five arbiters, of whom the author was one; and their report, dated in May 1798, is given as far as relates to the duties of the engines. The general average of twenty-three engines was 17,671,000. Since that period, so great have been the improvements in the economy of fuel and other parts of the machinery, that in December 1829, the duty of the best engine, with a cylinder of 80 inches, was 75,628,000, exceeding the duty performed in 1795, in the proportion of 3·865 to 1; and that of the atmospheric engine of 1778, in the proportion of 10·75 to 1.

The remainder of the paper relates to the friction in machinery, and the different modes of obviating its effects. With a view of reducing the amount of friction, the author is led to consider what are the most proper forms for the teeth and cogs of wheels; and through what intermediate steps a given increase of angular velocity may be

most advantageously communicated. Equability of velocity is obtained, though at the expense of some degree of sliding friction, when the outline of the teeth of the wheels are involutes of circles. Friction, on the other hand, is wholly prevented when their form is the logarithmic spiral; but the angular velocities will then be variable. Hence these two advantages are incompatible with one another; but on the whole, the author gives the preference to the involute, which produces an equability of angular motion. The most advantageous mode of increasing velocity by a series of wheels is to adjust them so that the multiplication of velocity shall proceed in a geometrical progression.

On the Laws of the Polarization of Light by Refraction. *By* David Brewster, *LL.D. F.R.S. L. & E.* Read February 25, 1830. [*Phil. Trans.* 1830, *p.* 133.]

M. Arago had deduced from some experiments which he made on the polarization of light with plates of glass, that the quantity of light polarized by reflexion is equal to the quantity polarized by transmission, whatever be the angle of incidence. The author of the present paper shows that the views from which this deduction was made, and the observations on which they are founded, are incorrect. By applying to the subject the same principles which he has already developed in the paper lately read to the Society, on the Polarization of Light by Reflexion, he establishes, on the basis of actual experiment, what he conceives to be the true laws of the phenomena. The first step in his inquiry is the determination of the law according to which the polarizing force of the refracting surface changes the position of the planes of polarized light. He shows that when a compound pencil, of which the constituent rays are polarized in planes inclined at angles of 45°, the one being to the right and the other to the left of the plane of refraction, is refracted, the planes of polarization of the refracted rays are turned so as to approach to coincidence, not in a plane parallel to the plane of reflexion, as happens in the reflected rays, but in a plane at right angles to it. This contrariety of effect, he observes, is exactly what might have been expected from the opposite character of the resulting polarization, the poles of the particles of light, which were formerly repelled by the force of reflexion, being now attracted by the refracting force. The author next endeavoured to ascertain the influence of refracting power in effecting polarization, but experienced great difficulty in prosecuting this inquiry, from the necessity of having plates without any crystalline structure. He tried gold leaf in a variety of ways, but found it almost impossible to obtain correct results, on account of the light which was transmitted unchanged through its pores. From observations with films of soapy water, and thin plates of metalline glass of high refractive power, he concludes that the rotation of the planes of polarization increases with the refractive power. By an examination of the effects produced at different angles of incidence, he deduces

the law expressing the variation of the rotation corresponding to the deviation of the refracted ray, when the inclination of the planes of polarization to the plane of incidence is 45°; namely, that the co-tangent of the inclination of the plane of polarization to that of refraction, is equal to the cosine of the difference between the angles of incidence and refraction. This formula represents the experiments so accurately, that when the analysing rhomb of calcareous spar is set to the calculated angle of inclination, the extraordinary image completely disappears; a result which is the strongest test of the correctness of the formula.

In order to determine the quantity of polarized light in the refracted pencil, the author follows a method similar to that which he employed for the reflected rays, and which he has explained in his former paper. He deduces as a general result, that the quantity of light polarized by refraction can never be mathematically equal to the whole of the transmitted pencil, however numerous be the refractions which it undergoes; or, in other words, refraction cannot produce rays truly polarized, that is, with their planes of polarization parallel. The same conclusions as were deduced in a preceding paper, respecting the partial polarization of light by reflexion, hold good with regard to similar changes produced by its refraction. Each refracting surface produces a change in the position of the planes of polarization, and consequently a physical change upon the transmitted pencil by which it has approached to the state of complete polarization. This proposition the author illustrates by applying the formulæ to the results of actual experiment, and showing their coincidence.

By prosecuting this investigation, the author arrives at the following important laws, namely, that at the first surface of all bodies, and at all angles of incidence, the quantity of light polarized by refraction is equal to the quantity polarized by reflexion; and also that the reflected is equal to the transmitted light when the inclination of the planes of polarization of the reflected pencil to the plane of the reflexion is the complement of the inclination of the planes of polarization to the same plane.

On the Action of the Second Surfaces of transparent Plates upon Light. By David Brewster, *LL.D. F.R.S. L. & E.* Read February 25, 1830. [*Phil. Trans.* 1830, *p.* 145.]

M. Arago had conceived that he had proved by an experiment, that at every possible angle of incidence the quantity of light polarized by reflexion was precisely equal to that of the light at the same time polarized by refraction. Dr. Brewster shows in the present paper, that the experiment does not warrant this conclusion; as the phenomena observed from it are the complicated effects of various refractions and reflexions from both surfaces of the glass, each affecting the position of the planes of polarization. By varying the form of the experiment in a way which allowed of the observation of these effects when separate, he is led to the following general law; namely,

that a pencil of light reflected from the second surface of a transparent plate, and reaching the eye after two refractions and an intermediate reflexion, contains, at all angles of incidence, from zero to the maximum polarizing angle, a portion of light polarized in the plane of reflexion. Above the polarizing angle, the part of the pencil polarized by reflexion diminishes until the cosine of the sum of the angles of incidence and reflexion equals the cube of the cosine of the difference between these two angles, when it disappears, and the whole pencil has the character of common light. Above this last angle, the pencil contains a quantity of light polarized perpendicularly to the plane of reflexion, which increases to a maximum, and then diminishes to zero, when the angle has attained 90°. The effect of the two refractions in M. Arago's experiment, was to make the two quantities of light appear equal, when in fact the one was exactly double of the other.

The paper concludes with formulæ and tables for computing the exact quantities of polarized light at all angles of incidence.

Observations made with the Invariable Pendulum (*No.* 4. Jones), *at the Royal Observatory, Cape of Good Hope, for the purpose of determining the Compression of the Earth. By the Rev.* Fearon Fallows, *F.R.S. Astronomer of the Cape Observatory. Communicated by the Lords Commissioners of the Admiralty.* Read February 18, 1830. [*Phil. Trans.* 1830, *p.* 153.]

Of the two methods employed for determining the figure of the earth, namely, the direct measurements of arcs of the meridian, or of ascertaining the variations in the length of the seconds pendulum in different places, the author remarks that the former is attended with the collateral benefit of fixing the geographical position of certain stations in the country surveyed; but the latter possesses the advantage of enabling the observer to concentrate, under his own immediate eye, the results of his inquiries. The Observatory at the Cape of Good Hope having been furnished by the Lords Commissioners of the Admiralty with the invariable pendulum of Jones, which had for several years been strictly examined by Capt. Sabine, the author was anxious to begin a series of experiments with it; and as it was not likely that the observatory would be completed for a considerable time, he caused a strong brick pier to be built in an adjoining outhouse for the support of a transit instrument, the same which he had used in forming his catalogue of southern stars. He gives a detailed account of his mode of fitting up the clock, and other parts of the apparatus necessary for the pendulum experiments. He was ably assisted by Capt. Ronald and Lieut. Johnson, who took an active part in all the observations. He remarks, that the near agreement of the three independent series of observations, made by himself and these two gentlemen, and which accompany the paper, is no small argument in favour of their accuracy. The difference in the number of vibrations of the seconds pendulum at the Cape, from that

in London, in a mean solar day, he finds to be 67·12, from which it results that the compression of the earth is $\frac{1}{\text{[illegible]}}$.

The author is of opinion that the invariable pendulum ought to be a standard instrument in every observatory; that it should be swung at all seasons of the year, and occasionally transferred to various fixed observatories in both hemispheres, and returned again to its original station, where it should undergo a renewed and rigid examination before it is sent round on a fresh circuit of these stations.

To this paper a note is subjoined by Capt. Sabine, containing a correction of the result obtained by Mr. Fallows, resulting from the application of the true elements of reduction for buoyancy and expansion, as stated in his late paper in the Philosophical Transactions, which had not reached the Cape when Mr. Fallows made his computations. The result of this correction gives 67·15 vibrations instead of 67·12. But when the observations of Capt. Ronald in London are taken in conjunction with those of Capt. Sabine, the retardation at the Cape is brought back to the exact number stated by Mr. Fallows.

Statement of the principal Circumstances respecting the united Siamese Twins now exhibiting in London. By George Buckley Bolton, *Esq. Member of the Royal College of Surgeons, and of the Medical and Chirurgical Society of London. Communicated by the President.* Read April 1, 1830. [*Phil. Trans.* 1830, *p.* 177.]

The twin brothers, of whom an account is given in this paper, were born of Chinese parents in 1811, at a small village in Siam, distant about sixty miles from Bankok, the capital of the kingdom. When the intelligence of their birth had reached the ears of the King of Siam, he gave orders that they should be destroyed, as portending evil to his government; but on being assured that they were harmless, and would be capable of supporting themselves by their own labour, he changed his intention, and suffered them to live. About six years ago Mr. Robert Hunter, a British merchant resident at Siam, saw them, for the first time, in a fishing-boat on the river, in the dusk of the evening, and mistook them for some strange animal. It was only in the spring of last year that permission could be obtained from the Siamese Government to bring them to England. They were taken to Boston, in the United States, where they landed in August last, and six weeks afterwards embarked for England, and arrived in London in November. They are both of the same height, namely, five feet two inches, and their united weight is 180 pounds. They have not the broad and flat forehead so characteristic of the Chinese race, but they resemble the lower class of the people of Canton in the colour of their skins and the form of their features. Their bodies and limbs are well made. The band of union is formed by the prolongation and junction of the ensiform cartilages of each, which meet in the middle of the upper part of the band, and form moveable joints with each other, connected by ligamentous structures. Under-

neath the cartilages there appear to be large hernial sacs opening into each abdomen, into which, on coughing, portions of the intestine are propelled, as far as the middle of the band; though in ordinary circumstances these herniæ are not apparent. The entire band is covered with common integument; and when the boys face each other, its length at the apex is one inch and three quarters, and at the lower edge not quite three inches. Its breadth from above downwards is four inches, and its greatest thickness nearly two inches. In the centre of the lower edge there is a cicatrix of a single navel. It possesses little sensibility, and is of great strength; for upon a rope being fastened to it, the twins may be pulled along without occasioning pain; and when one of them is lifted from the ground, the other will hang by the band alone without sensible inconvenience. For the space of about half an inch from the median line of the band, the sensibility of the skin appears to be common to both. The following experiment was tried upon them by Dr. Roget. A silver tea-spoon being placed on the tongue of one of the twins, and a disk of zinc on the tongue of the other, the moment the two metals were brought into contact, both the boys exclaimed "Sour, sour;" thus proving that the galvanic influence passed from the one to the other through the connecting band.

Their strength and activity are very remarkable. They can throw down, with perfect ease, a powerful man. They run with great swiftness, bend their bodies in all directions, and in their sports often tumble head over heels without the least difficulty or inconvenience. In all the bodily actions in which the concurrence of both is required, such as running, jumping, playing at battledoor and shuttlecock, they exhibit a wonderful consent or agreement without the appearance of any previous communication of their intentions. The intellectual powers of each are nearly equal, and they have both attained the same degree of proficiency in the games of chess, draughts, and whist. They both possess great powers of imitation. In their respective physical constitutions, however, several differences are observable. Chang, as the boy on the left is named, has more vigorous health, and greater regularity of functions, than his brother, whose name is Eng. In general they take their meals and obey the calls of nature at the same time. Asparagus, eaten by either of the twins, communicates its peculiar odour exclusively to the urine of the one who has eaten it.

The author details the circumstances of a catarrhal complaint which attacked both of them in December last, the symptoms and progress of which were similar in both, and from which they both recovered in the same manner and at the same time. In their healthy state their ordinary pulses are generally alike, and are easily excited; but that of the one may be accelerated, while that of his brother continues calm.

In their habits they are very cleanly and delicate; in their dispositions affectionate, and grateful for every kindness shown to them.

There exists between them the most perfect harmony. They always fall asleep at the same moment, and it is impossible to wake the one without also waking the other.

The author adverts, in the course of the paper, to the question whether they were the produce of a single or a double ovum; and also into that of the possibility, at some future time, of effecting their separation with safety to themselves; and he concludes by bearing testimony to the uniform kind treatment they have received from Capt. Coffin, Mr. Hunter, and Mr. Hale, who have evinced on all occasions the greatest anxiety for their welfare and happiness; and to the liberal manner in which they have always afforded access to men of science for promoting any object of philosophical inquiry.

On some Properties in Achromatic Object-glasses applicable to the Improvement of the Microscope. By Joseph Jackson Lister, *Esq. Communicated by Dr.* Roget, *Secretary.* Read January 21, 1830. [*Phil. Trans.* 1830, *p.* 187.]

The principles on which the reflecting, and also the achromatic refracting telescope are constructed, have been recently applied with considerable success to the microscope, and have added much to the power of that instrument. The author speaks with much commendation of the peculiar construction adopted in Mr. Tulley's compound achromatic microscopes, consisting of a combination of object-glasses of short focus and large aperture, the curvatures of which are such as very nearly to equalize the refractions produced by each. As the magnitude of the aperture, he observes, is valuable only in proportion to that of the pencil of light which it admits, the latter circumstance is that which chiefly claims attention; and as it is often erroneously estimated, a method is pointed out of ascertaining it with sufficient exactness for every practical purpose. He then enters into a detailed description of the several parts of an instrument in his possession constructed on the principles he recommends, referring to the drawings which accompany the paper. The magnifying power may be varied at pleasure, either by drawing out the tubes containing the eye-pieces, or by substituting an eye-glass of different power, or differently combined; and by these changes an uninterrupted range of amplification is obtained from 35 to 800 diameters. No sensible difference as to distinctness is observable, whether the effect is produced by changing the eye-piece, or varying the length of the tubes. The construction of the instrument admits of the utmost variation of magnifying power without the risk of losing sight of the object viewed: and every part which relates to the illumination being wholly detached from the stage, ample opportunity is afforded of rapidly moving the objects, and bringing into view a succession of them, while the light remains the same. Minute directions are given for the employment of the instrument, and its application to various purposes; and great stress is laid on the importance of a skilful management of the light.

In stating the results of his experience on this subject, the author takes occasion to advert to some of the sources of fallacy by which incautious observers with the microscope have so often been greatly misled. When a pencil of rays proceeding from an indefinitely small bright portion of an object is brought to a focus by the most perfect object-glass, the image thus formed is in reality not a point, but a small circle, and will always appear as such if the eye-glass of the microscope be sufficiently powerful. These circles have a considerable analogy to the spurious discs of stars viewed through telescopes. Like the latter, they become much enlarged by diminishing the aperture of the object-glass; and they are also enlarged by increasing the intensity of the illumination. The overlapping of contiguous circles of diffusion has given rise to many fallacious appearances; such as the spottiness which some surfaces assume, and which has been mistaken for globules. This optical illusion has been the basis of some ingenious but visionary speculations on the intimate structure of organic matter. The appearance, in certain directions of the light, of lines on the surface of an object, where they do not really exist, may be traced to a similar cause.

The author proceeds to describe the method he uses for measuring the dimensions of the objects viewed, and notices different test objects with reference to their affording the means of judging of the powers of the instrument. He next enters into a review of the comparative merits of various microscopes constructed by Cuthbert and Dollond in this country, and by Chevalier, Selligue, Amici, Utzschneider, and Fraunhofer, on the continent.

The concluding part of the paper is occupied by the developement of a principle, from the application of which to the construction of the microscope, the author expects that a still greater extension of its powers will ere long be obtained. He remarks, that the circumstance which limits the magnitude of the pencil of light, admissible with high powers by a single achromatic object-glass, is, that the correction for spherical aberration by the concave lens is proportionally greater for the rays that are remote from the centre, than for the central rays. The degree of confusion in the image, thence arising, is, in similar glasses, inversely as the square of their focal lengths. It increases very rapidly with a small enlargement of the aperture, but may be rendered much less considerable by distributing the refractions equally among a greater number of lenses of smaller curvature. Hence the advantage obtained by certain combinations. The experiments made by the author have established the fact, that in general an achromatic object-glass, of which the inner surfaces are in contact, will have on one side of it two aplanatic foci in its axis, for the rays proceeding from which it will be truly corrected with a moderate aperture; that for those proceeding from any part of the interval between these two points, the spherical aberration will be over-corrected; and that for rays beyond these limits it will be under-corrected. Methods are pointed out for ascertaining the situation of these aplanatic foci. The principle here explained furnishes

the means of destroying both kinds of aberration in a large focal pencil, and of thus surmounting what has hitherto been a chief obstacle to the perfection of the microscope.

On the Pendulum. By J. W. Lubbock, *Esq. F.R.S.* Read March 11, 1830. [*Phil. Trans.* 1830, *p.* 201.]

The ingenious and beautiful application, made by Capt. Kater, of Huygens's theorem respecting the convertibility of the centres of suspension and oscillation, to the determination of the length of the simple pendulum, is to be considered as a first approximation to the solution of this problem. The accuracy of this determination, however, may be affected by many circumstances which the theory does not take into account; and the object of the author in this paper is to investigate the limits of the errors that may arise from neglecting them. Laplace and Whewell have shown that when the knife-edges are considered as cylinders of small but of equal radii of curvature, their distance is still equal to the length of the simple pendulum. The author treats the question with the utmost generality, and discusses all the circumstances which may affect the accuracy of Capt. Kater's method, including all possible deviations and positions of the axes. He takes, as an example, the pendulum used by Mr. Baily, and described by him in the Philosophical Magazine of last February; and investigates the errors which would arise in the length of the simple pendulum corresponding to given deviations of the knife-edges. He also considers the case in which the agate planes are fixed on the pendulum, and vibrate on a fixed knife-edge; and finds that the length of the simple pendulum is here also equal to the distance between the planes.

On the Theoretical Investigation of the Velocity of Sound, as corrected from M. Dulong's *recent Experiments, compared with the Results of the Observations of Dr.* Moll *and Dr.* Van Beek. *By Dr.* Simons, *Assistant at the Observatory of the University of Utrecht. Communicated by Captain* Henry Kater, *Vice-President.* Read March 18, 1830. [*Phil. Trans.* 1830, *p.* 209.]

Laplace has demonstrated that Sir Isaac Newton's formula for obtaining the velocity of sound, requires, in order to render it correct, that it be multiplied by a certain co-efficient, depending on the ratio between the specific heats of atmospheric air under a constant pressure, and under a constant volume. Laplace has endeavoured to deduce this coefficient, first from the experiments of MM. De la Roche and Berard; secondly, from those of MM. Clement and Desormes; and lastly, from the more accurate investigations of MM. Gay-Lussac and Welter. By applying this correction, the velocity of sound, deduced from calculation, corresponded very nearly with the results of actual experiment. Still, however, a degree of discordance was always found to take place. With a view to perfect

the theory still further, Dulong attempted, by reversing the process of Laplace, to deduce the coefficient by which the Newtonian formula is to be multiplied, directly from experiments themselves. The object of the present paper is to compare the investigation of Dulong with the experiments on the velocity of sound made by Drs. Moll and Van Beek, of which an account was lately published in the Philosophical Transactions. By applying the values of the coefficients thus obtained, the computed velocities of sound came out much nearer to the observed velocities; and the author concludes by remarking, that such differences as yet remain between calculation and experiment, may with great probability be ascribed to the errors, which are unavoidable in observations of so complicated a nature.

On the Elasticity of Threads of Glass, with some of the most useful Applications of this property to Torsion Balances. By William Ritchie, *A.M. F.R.S., Rector of the Royal Academy of Tain.* Read March 18, 1830. [*Phil. Trans.* 1830, *p.* 215.]

The author proposes the employment of threads of glass in the construction of torsion-balances, in place of the silver wire, used by Coulomb for the measurement of minute electric or magnetic forces. He describes a galvanometer of his invention, acting upon this principle, the intensity of the galvanic current being measured by the torsion of a slender filament of glass, to the lower end of which a magnetized needle is fixed at right angles. He also applies the same power to the improvement of the sensibility of the common balance for weighing minute bodies, by affixing to the beam a long glass thread horizontally in the axis of suspension, by the torsion of which, when the balance has been brought nearly to a level, the more accurate adjustments are to be effected. On the whole he considers that glass, from its perfect elasticity, possesses decided advantages over metallic wires, for the construction of instruments acting on the principle of torsion.

Memoir on the occurrence of Iodine and Bromine in certain Mineral Waters of South Britain. By Charles Daubeny, *M.D. F.R.S. Professor of Chemistry in the University of Oxford.* Read May 6, 1830. [*Phil. Trans.* 1830, *p.* 223.]

The author lays claim to being the first who announced to the public the existence of bromine in the mineral springs of England; a discovery similar to that which had been previously made by others in many analogous situations on the Continent. His reason for offering the present communication to the Royal Society is, that he has examined on the spot a great number of mineral springs, and endeavoured to obtain, wherever it was practicable, an approximation to the proportion which iodine and bromine bear to the other ingredients. He has also aimed at forming an estimate of their comparative frequency and abundance in the several rock formations, an ob-

ject of considerable interest in geology, as tending to identify the products of the ancient seas in their most minute particulars with those of the present ocean. The results of his inquiries are given in the form of a table, in which the springs, whose waters he examined, are classified according to the geological position of the strata from which they issue, and of which the several columns exhibit the total amount of their saline ingredients; the nature and proportion of each ingredient, as ascertained by former chemists, or by the author himself; and lastly, where they contained either iodine or bromine, the proportions these substances bear to the quantities of water, and likewise to the chlorine also present in the same spring. He finds that the proportion of iodine to chlorine varies in every possible degree; and that even springs which are most strongly impregnated with common salt are those in which he could not detect the smallest trace of iodine. The same remark, he observes, applies also to bromine; whence he concludes, that although these two principles may, perhaps, never be entirely absent where the muriates occur, yet their relative distribution is exceedingly unequal. The author conceives that these analyses will tend to throw some light on the connection between the chemical constitution of mineral waters and their medicinal waters. Almost the only two brine springs, properly so called, which have acquired any reputation as medicinal agents, namely, that of Kreutznach in the Palatinate, and that of Ashby-de-la-Zouch in Leicestershire, contain a much larger proportion than usual of bromine, a substance, the poisonous quality of which was ascertained by its discoverer Balard. The author conceives that these two recently discovered principles exist in mineral waters, in combination with hydrogen, forming the hydriodic and hydrobromic acids, neutralized in all probability by magnesia, and constituting salts which are decomposable at a low temperature. He has no doubt that a sufficient supply of bromine might be procured from our English brine springs, should it ever happen that a demand for this new substance were to arise.

Experiments to determine the Difference in the Number of Vibrations made by an Invariable Pendulum in the Royal Observatories of Greenwich and Altona. By Captain Edward Sabine, *of the Royal Artillery, Sec. R.S.* Read March 25, 1830. [*Phil. Trans.* 1830, *p.* 239.]

The invariable pendulum, No. 12, with which the experiments recorded in this paper were made, was vibrated in the Royal Observatory at Greenwich in July 1828; in the Royal Observatory at Altona in September and October of the same year; and again at the Royal Observatory at Greenwich in August 1829. The mean of the results obtained at Greenwich in July 1828 and in August 1829, give the rate of this pendulum at Greenwich to be compared with its rate obtained at Altona. The details of all these series of observations are given in a tabulated form.

Experiments to ascertain the Correction for Variations of Temperature, within the limits of the natural Temperature of the Climate of the South of England, of the Invariable Pendulum recently employed by British Observers. By Captain Edward Sabine, *of the Royal Artillery, Sec. R.S.* Read March 25, 1830. [*Phil. Trans.* 1830, *p.* 251.]

The correction for temperature which the author deduces as the general result of his investigation, is 0·44 of a vibration *per diem* for each degree of Fahrenheit between 30° and 60°. He considers this result as entitled to the greater confidence, from the favourable nature of the circumstances under which the inquiry was conducted; since the influence of natural temperature is more permanent and equable than that of temperatures artificially produced. He considers it as desirable, however, that means should be devised of extending experiments on this subject to a wider range of temperatures.

On a new Register-Pyrometer, for measuring the Expansions of Solids, and determining the higher Degrees of Temperature upon the common thermometric Scale. By J. Frederic Daniell, *Esq. F.R.S.* Read June 17, 1830. [*Phil. Trans.* 1830, *p.* 257.]

In the year 1821, the author published in the Journal of the Royal Institution an account of a new pyrometer, and of some determinations of high temperatures, in connexion with the scale of the mercurial thermometer, obtained by its means. The use of the instrument then described was, however, limited; and the author was subsequently led to the invention of a pyrometer of a more universal application, both to scientific researches and to various purposes of art. He introduces the subject by an account of the late attempt of M. Guyton de Morveau, to employ the expansions of platina for the admeasurement of high temperatures, and for connecting the indications of Wedgwood's pyrometer with the mercurial scale, and verifying its regularity. The experiments of that philosopher were by the contraction of porcelain, and by actual comparison with those of the platina pyrometer, at no higher temperature than the melting point of antimony; but they are sufficient to establish the existence of a great error in Wedgwood's original estimation of his degrees up to that point. This he carries on by calculation, on the hypothesis of uniform progression of expansion, up to the melting point of iron; the construction of his instrument not admitting of its application to higher temperatures than a red heat, in which platina becomes soft and ductile. Mr. Daniell shows, by an examination of M. Guyton's results, that he has failed in establishing the point he laboured to prove; namely, the regularity of the contraction of the clay pieces.

The pyrometer of the author consists of two distinct parts; the one designated the *register*, the other the *scale*.

The first is a square tube of black-lead, 8 inches long, cut out of a common crucible of that material, closed at one end, and having at the other a portion of about six tenths of an inch in length cut away

to the depth of half the diameter of the bore, so as to leave a shoulder near the end. A bar of any metal $6\frac{1}{2}$ inches long is introduced into the cavity, resting against its solid end, and a cylindrical piece of porcelain, about $1\frac{1}{2}$ inch long, which he calls the index, is placed upon the top of the bar, and projects beyond the open part of the tube, being confined in its place by a ring or strap of platina passing round it, and also round the end of the black-lead bar, and made sufficiently tight by a small porcelain wedge inserted between them. When the instrument thus prepared is subjected to heat, the porcelain index will be forced up, by the expansion of the metallic bar, to a certain distance, where it will remain when the bar retires from it on cooling. The distance it has been moved from its original position, will be the measure of the difference of expansion of the metallic bar, and of an equal length of the black-lead in which it is contained. This cannot be influenced by any permanent contraction which the black-lead may undergo by intense heat, because any such contraction will occur at the moment of the greatest expansion of the metal, and the index will still mark its point of furthest extension upon this contracted basis. It remains then to measure accurately the distance to which the index has been moved by the application of the scale, which is a detached instrument constructed of two rules of brass joined together at a right angle; the one fitting square upon two sides of the black-lead bar, the other resting on its shoulder; with these are connected two arms, which, acting on the principle of proportional compasses, measure the distance of the extremity of the index from the shoulder of the black-lead bar. The spaces comprehended between the points of the shorter legs of the compasses are magnified ten times by the longer legs, the angular motion being measured by a graduated arc furnished with a vernier, and capable of being easily read off to minutes.

The author next enters into a comparison of the results afforded by this instrument with those of former experimentalists; and especially with the accurate determination of the expansions of metals by Messrs. Dulong and Petit, with a view to show the degree of confidence to which it is entitled. The close agreement in the results of a great number of experiments upon metals which differ much in their expansions is highly satisfactory in this respect. Differences having been found in the expansibility of different specimens of black-lead, it becomes necessary to ascertain the expansions of each register for itself, by applying to it the heat of boiling mercury. The author concludes with an account of some experiments which he made to determine the fusing points of different metals, referred to the common thermometric scale. His calculations proceed upon the supposition that the equal amounts of expansion denote equal increments of temperature; and he is thus enabled to compare the series now obtained with that which was obtained with his first pyrometer. A remarkable accordance is found between the results with platina and with iron,—metals which differ widely in their expansions; conformably with the conclusions of MM. Dulong and Petit, the expansion

of iron increases at higher temperatures in a greater ratio than that of platina. The discrepancy between the temperatures derived from the observations with his first pyrometer and the present one, he admits to be considerable, but believes they may be sufficiently accounted for by the differences in the circumstances of the experiments, without imputing inaccuracy to the instrument. The author next attempted to ascertain the effects of the most intense heat which it was possible to produce in a furnace, and to measure the utmost limits of expansion in a platina bar; but various circumstances interfered with the success of these experiments, which afforded, however, many curious results as to changes of integration in platina by the effects of heat.—The paper concludes with some observations on the practical advantages possessed by the present instrument.

On the Phenomena and Laws of Elliptic Polarization, as exhibited in the Action of Metals upon Light. By David Brewster, *LL.D. F.R.S. L. & E.* Read April 22, 1830. [*Phil. Trans.* 1830, *p.* 287.]

The action of metals upon light has always presented a remarkable, and hitherto inexplicable, anomaly in the science of polarization. Malus, to whom this branch of optics owes its origin, had at first announced that metals exerted no polarizing influence on light; but Dr. Brewster, by employing a different method of observation, ascertained that the light reflected from metallic surfaces was modified in such a manner as to exhibit, when transmitted through thin crystallized plates, the complementary colours of polarized light. He afterwards discovered the curious property possessed by silver and gold, of dividing a polarized ray into complementary colours by successive reflexions. M. Biot, to whom the author communicated this discovery, pursued the inquiry to which it led, and arrived at the same conclusions as to the mode in which this class of phenomena should be explained. Subsequent researches, however, convinced the author that these generalizations had been too hastily formed; and the study of Fresnel's curious discoveries respecting circular polarization enabled him to advance still further in the inquiry, and he now presents to the Royal Society in this paper, a complete analysis of the singular phenomena exhibited in the action of metals upon light.

The first section of the paper treats of the action of metals upon common light. A ray of common light reflected from a metallic surface when analysed by a rhomb of calcareous spar, exhibits a defalcation of light in one of the images, as if a portion of the light was polarized in the plane of reflexion. This effect will be still more distinctly seen on examining the system of polarized rings formed round the axes of crystals by means of the light reflected from metals. If the light had suffered no modification by reflexion, or if the metal reflected in equal quantities the light polarized in opposite planes, the rings would not be visible at all, whereas it is found that they are easily visible in the light reflected from all metals. They are most distinctly perceived at an incidence of about 74°, and be-

come more and more faint as the incidence succeeds or falls short of that angle. They appear best defined in light reflected from galena, and from metallic lead, and with least distinctness in light reflected from silver and gold. On examining the effect of successive reflexion of the same ray by metallic surfaces, the author found that the quantity of light which each polarizes in the plane of reflexion, increases with every reflexion, and that in several cases the whole incident pencil is completely polarized.

The action of metals upon polarized light forms the subject of the second section of this paper, in which he investigates the changes which polarized light undergoes, according as it is reflected at different angles of incidence, and in different azimuths of the plane of incidence with relation to the plane of primitive polarization. The light experiences in these cases a physical change of a nature intermediate between that of completely polarized light, and light wholly unpolarized, neither does it possess the same characters as that which has passed through thin crystallized plates. Its constitution is exceedingly analogous to light which is circularly polarized; that is, which comports itself as if it revolved with a circular motion during its transmission through particular media. But in the case of circular polarization, the ray has the same properties in all its sides, and the angles of reflexion at which it is restored to simple polarized light in different azimuths, are all equal, like the radii of a circle described round the ray. In the case of metallic reflexions, the new phenomena discovered by Dr. Brewster may be designated by the term *elliptic polarization*, because the angles of reflexion at which this kind of light is restored to polarized light may be represented by the variable radius of an ellipse. In circular polarization the restored ray has its plane of polarization always inclined — 45° to the plane of the second system of reflexion. In elliptic polarization, the inclination of the plane of the restored pencil is always less than 45°. In the former case, this plane continues by successive reflexions to oscillate on each side of the plane of reflexion, with a never-varying amplitude, from + 45° to — 45°; while in the latter case the same plane oscillates with an amplitude continnally diminishing till it is brought to zero in the plane of reflexion. In steel, the polarization is highly elliptical, and the amplitude of the oscillations of the plane of restoration is quickly brought to zero; but in silver, whose polarization approaches nearly to circular, the oscillations diminish very slowly in amplitude. The peculiar character of elliptic polarization shows itself also in another manner in the variable position of the ellipses which regulate its angles of restoration upon steel.

In the third section of his paper, the author treats of the complementary colours produced by successive reflexion from the polished surfaces of metals.

He concludes by observing, that although we do not understand the nature of the forces by which metals reflect the two oppositely polarized pencils, yet we are certain they do not act exactly in the same manner as the second surfaces of transparent bodies, when pro-

ducing total reflexion. Setting out from a perpendicular incidence, the least refrangible rays begin to suffer the double reflexion sooner than the mean ray, and they sooner reach their maximum of elliptic polarization, thus exhibiting the inversion of the spectrum. The theory of circular polarization, as given by Fresnel, will no doubt embrace the phenomena of elliptic polarization; and when the nature of metallic action shall be more thoroughly examined, we may expect to be able to trace the phenomena under consideration to their true source.

Researches in Physical Astronomy. By John William Lubbock, *Esq. F.R.S.* Read April 29, 1830. [*Phil. Trans.* 1830, *p.* 327.]

The analytic expressions for the variations of the elliptic constants given by Laplace in his *Mécanique Céleste*, are true only when the square and higher powers of the disturbing forces are neglected in the computation; and by proceeding on the supposition that all the planets move in circular orbits and in the same direction, he has demonstrated that the eccentricities and inclinations vary within small limits, and that the stability of the planetary system is always eventually preserved. But Mr. Lubbock shows in the present paper that these conditions are not necessary to the stability of a system of bodies subject to the law of attraction which governs our system; and he gives expressions for the variations of the elliptic constants which are rigorously true, whatever power of the disturbing force be retained.

On the Error in Standards of Linear Measure, arising from the thickness of the Bar on which they are traced. By Captain Henry Kater, *V.P. and Treas. R.S.* Read June 17, 1830. [*Phil. Trans.* 1830, *p.* 359.]

While engaged in the adjustment and verification of the copies of the Imperial standard yard destined for the Exchequer, Guildhall, Dublin, and Edinburgh, the author discovered a source of error arising from the thickness of the bar, upon the surface of which measures of linear dimension are traced. A notice to that effect was published in the Philosophical Transactions for 1826; and the object of the present paper is to give an account of the experiments the author has since made on this subject, and to describe a scale which he has had constructed, so as almost entirely to obviate the source of error thus introduced.

From the experiments detailed in the first part of the paper, the following conclusions are deduced:—First, that in a standard of linear measure, traced upon the surface of a bar, an error arises from the thickness of the bar when it is placed upon a table the surface of which is not plane; Secondly, that this error in bars of the same material, and of unequal thickness, is within certain limits as the thickness of the bar, and depends upon the extension of that sur-

face of the bar which becomes convex, and the compression of the surface which is concave; Thirdly, that the error to which the same scale is liable from this cause, is directly as the versed sine of the curvature of the surface upon which the scale is placed; Fourthly, that the error very far exceeds that which would arise from the difference of length between the arc and its chord under similar circumstances; so much so, that the sum of the errors from this cause, in a bar one inch thick, with a versed sine of not one hundredth of an inch, is nearly one thousandth of an inch, whilst double the difference between the chord and the arc is not one fifty thousandth.

The author devised the following method of trying a surface supposed to be plane; namely, by applying to it in different directions a piano-forte wire, one hundredth of an inch in diameter, which bears a considerable degree of tension without breaking, strung on a bow 6 feet long; a contrivance which, he states, may be applied to a great variety of useful purposes, when a straight edge is required. He could detect the nature, and in some degree the extent, of the irregularities of a surface, by tapping with the fingers upon the wire whilst it was pressed by the weight of the bow upon the board. When it yielded no sound, the wire was of course in contact with the surface, which was, in that case, either convex or plane. When the wire yielded a sound the surface was concave, and some idea might be formed of the extent by the acuteness or gravity of the sound produced, the edges of the concavity serving as bridges which limited the length of the string. So delicate is this test, that a cavity can be detected by this method when the interval between the wire and the surface under examination is imperceptible to the eye.

The error in question, resulting from the extension and compression of the surfaces of the bar dependent upon its curvature, is obviated in the following manner. The neutral surface, which suffers neither extension nor compression, is shown by the author to be at about one third of the thickness of the bar from that surface, which becomes convex. When the object is to have two points only on the bar, marking for example the yard, by cutting away one half of the thickness of the bar at its ends, and placing the points upon the new surface, the error arising from flexure is reduced to the least possible quantity; but when a scale of inches is required, the nearest approximation to correct measurement is obtained by diminishing as much as possible the thickness of the bar, and by providing another bar on which it is to be supported, and on which it is allowed to slide freely in a dovetailed groove, formed by two side plates of similar thickness, screwed to the surface of the bar, and to which it is to be fixed at its middle point by a single screw passing through it.

On the Illumination of Light-houses. By Lieut. Thomas Drummond, *of the Royal Engineers. Communicated by Lieut.-Colonel* Colby, *of the Royal Engineers, F.R.S.* Read June 17, 1830. [*Phil. Trans.* 1830, *p.* 383.]

The author, after briefly describing the different methods at present employed for illuminating light-houses, proceeds to detail what he considers an improvement upon those now in use. This consists in substituting for the Argand burner a small ball of lime, ignited by the combustion of oxygen and hydrogen.

From this small ball, only three eighths of an inch in diameter, so brilliant a light is emitted, that it equals in quantity about 13 Argand lamps, or 120 wax candles; while in intensity, or intrinsic brightness, it cannot be less than 260 times that of an Argand lamp. These remarkable results are deduced from a series of experiments made lately at the Trinity House; and having been repeated with every precaution, and by different individuals, there seems no reason to doubt their accuracy.

In the best of our revolving lights, such as that of Beachy Head, there are no less than 30 reflectors, 10 on each side. If, then, a single reflector, illuminated by a lime ball, be substituted for each of these 10, the effect of the three would be 26 times greater than that of the 30. On account of the smaller divergence of the former, it would be necessary to double their number, placing them in a hexagon instead of a triangle; in this case, the expense is estimated at nearly the same. This method was tried lately at Purfleet, in a temporary light-house, erected for the purpose of experiments by the Corporation of the Trinity House; and its superiority over all the other lights with which it was contrasted, was fully ascertained and acknowledged.

On the evening of the 25th of May, when there was no moonlight, and the night dark, with occasional showers, the appearance of the light, viewed from Blackwall, a distance of ten miles, was described as being very splendid. Distinct shadows were discernible, even on a dark brick wall, though no trace of such shadows could be perceived when the other lights, consisting of 7 reflectors, with Argand lamps and the French lens, were directed on the same spot.

Another striking and beautiful effect, peculiar to this light, was discernible when the reflector was turned, so as to be itself invisible to the spectator. A long stream of rays was seen issuing from the spot where the light was known to be placed, and illuminating the horizon to a great distance. As the reflector revolved, this immense luminous cone swept the horizon, and indicated the approach of the light long before it could itself be seen from the position of the reflector. These singular effects must not, however, be understood as constant accompaniments of this light; for on a moonlight night, or when the weather is very hazy, they cease to appear.

On the Electro-magnetic Properties of metalliferous Veins in the Mines of Cornwall. By Robert Were Fox, *of Falmouth. Communicated by the President.* Read June 10, 1830. [*Phil. Trans.* 1830, *p.* 399.]

The author having been led from theory to entertain the belief that a connexion existed between electric action in the interior of the earth, and the arrangement of metalliferous veins, and also the progressive increase of temperature in the strata of the earth as we descend from the surface, proceeded to the verification of this opinion by experiment. His first trial was unsuccessful; but in the second he obtained decisive evidence of considerable electrical action in the mine of Huel Jewel in Cornwall. His apparatus consisted of small plates of sheet copper, which were fixed in contact with one in the veins by copper nails, or else wedged closely against them with wooden props stretched across the galleries. Between two of these plates at different stations, a communication was made by means of copper wire one twentieth of an inch in diameter, which included a galvanometer in its circuit. In some instances, 300 fathoms of copper wire were employed.

The intensity of the electric currents was found to differ considerably in different places; it was generally greater in proportion to the greater abundance of copper ore in the veins, and in some degree also to the depth of the stations. Hence the discovery of the author seems likely to be of practical utility to the miner in discovering the relative quantity of ore in veins, and the directions in which it most abounds. The electricity thus perpetually in action in mines does not appear to be influenced by the presence of the workmen and candles, or even by the explosion of gunpowder in blasting.

The author's experiments enable him to give a table of the relative powers of conducting galvanic electricity possessed by various metalliferous minerals. This power, he remarks, appears to bear no obvious relation to any of the electrical or other physical properties of the metals themselves, when in a proper state, or to the proportions in which they exist in combination. He proceeds to point out various facts relative to the position of veins, and the arrangement of their contents, which he thinks are irreconcileable with any of the hypotheses that have been devised to explain their origin.

He observes that ores which conduct electricity have generally some conducting substances interposed in the veins between them and the surface; a structure that appears to bear a striking analogy to the ordinary galvanic combinations. He is of opinion that the intensities both of heat and of electricity, and consequently of magnetism, increase in proportion to the depths of the strata under the surface of the earth; that they have an intimate connexion with one another; and that the discovery of electrical currents in various and frequently opposite directions, in different parts of the same mine, may perhaps, hereafter, afford a clue to explain the declination and variation of the magnetic needle.

Sequel to a Paper on the tendency to Calculous Diseases, and on the Concretions to which such Diseases give rise. By John Yelloly, *M.D. F.R.S. &c.* Read June 17, 1830. [*Phil. Trans.* 1830, *p.* 415.]

The author, in a paper published in the last volume of the Philosophical Transactions, gave the analysis of 328 calculi contained in the collection of the Norfolk and Norwich Hospital; and has been since enabled to complete the analysis of the 335 remaining specimens, which have now been divided. The results of the analysis are given in a tabular form, exhibiting in the order of their occurrence from the centre, the consecutive deposits of the different materials of which the calculi are composed, according to the most prominent character of such material. The most remarkable circumstance brought to light in the course of this investigation, is the discovery of the presence of silex in one specimen composed principally of oxalate of lime, and weighing about 5 grains. The particles of silex were very minute, and were imbedded in, and diffused through, the oxalate of lime. Three examples of a similar occurrence are quoted by the author.

The paper concludes with a few remarks on the statistical conclusions stated in his former communication. He thinks there is reason to believe that the average number of calculous disorders in Scotland has been much underrated; on the other hand, the proneness to those complaints is very small in Ireland. A much larger proportion of calculous cases occurs in towns than in the country.

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